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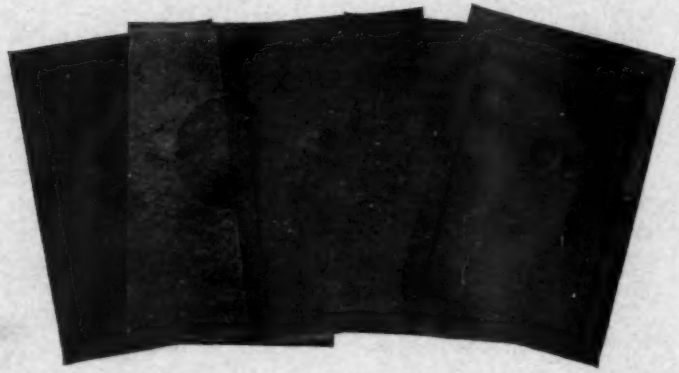

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FALSE SPECTRA FROM DIFFRACTION GRATINGS

PART I. SECONDARY SPECTRA.* BY W. F. MEGGERS AND C. C. KIESS
PART II. THEORY OF LYMAN GHOSTS. BY CARL RUNGE
PART III. PERIODIC ERRORS IN RULING MACHINES. BY J. A. ANDERSON

PART I. SECONDARY SPECTRA

1. INTRODUCTION

The ideal diffraction grating would be one which is free from all errors of ruling and in which the shape of the ruled groove is such that all the light is concentrated in a single spectrum. No such perfect gratings have ever been produced, although some very excellent instruments have been made and used since the epoch making work of Prof. Rowland. These instruments have contributed so enormously to the advance in spectroscopic knowledge, that it seems almost disrespectful to mention the errors into which they sometimes lead spectroscopists. However, there are at least two considerations in the use of diffraction gratings which call for extreme care and watchfulness to avoid spurious results.

One of these arises from the presence of different orders of spectra which overlap. This superposition of spectra has caused some spurious results[1] and it is probable that false data from this cause are even more abundant than has been admitted or exposed

* Published by permission of the Director of the Bureau of Standards.

[1] Hale, Explanation of a Supposed Abnormal Solar Spectrum, Astroph. Jour. *46*, p. 291; 1917.

Walters, B. S. Bulletin *17*, p. 173; 1921, Wave-Lengths of Antimony by Kretzer, Zeit. f. wiss. Phot., *8*, p. 56; 1910.

thus far. This difficulty which is inherent in the principle of the diffraction grating when the characteristics of the groove are such as to distribute the light among spectra of different orders can either be avoided by a careful choice of filters to remove overlapping orders, or the spurious lines can be detected by simple numerical relationships which they bear to the true spectra.

The second pitfall in the use of diffraction gratings has its origin in the periodic errors of ruling which are perhaps inevitable in machine made instruments. Errors from this source are not uncommon in spectroscopic literature[3] and more of them may exist than are now apparent. Spurious spectral lines arising from periodic ruling errors may also be regarded as superposed spectra of various orders, produced however by a secondary grating or gratings with different instrumental constants. In complex spectra such false lines or "ghosts" are indistinguishable from real lines and may occur in all regions of the spectrum with intensities equal to those of the fainter real lines. They can be identified only by their more or less complex numerical relationships to the true or parent lines, and can be predicted only if the constants of the superposed gratings are known. Although the superposed secondary spectra are usually much inferior to the primary spectrum in intensity they may nevertheless put in unexpected appearance and cause considerable embarrassment especially when unsafe filters are used or when long photographic exposures are made to record spectra in the ultra-violet or infra-red extremes of the spectrum.

A large concave grating which has been used for infra-red spectrum investigations at the Bureau of Standards since 1915 was found in 1919 to give types of secondary spectra, which had not been observed heretofore. This happened during an investigation of the spectrum of chromium when it was observed that strikingly characteristic groups of red lines were reproduced at several points in the infra-red. The ratios of these apparent wave-lengths to the real ones were then determined and applied to work pre-

[3] Kayser, Handbuch der Spectroscopie, I, p. 448.

Meggers and Kiess, B. S. Bulletin, *14*, p. 637; 1918. Note added April 16, 1919, p. 651.

viously done with this grating. Many lines already published for iron, nickel and cobalt[3] were found to be related to lesser wave-lengths by these ratios as was stated in the note added to that paper on April 16, 1919 (loc. cit., p. 651). This experience persuaded us to make a careful and thorough investigation of false spectra from diffraction gratings, a preliminary report[4] of which work was presented in Sept., 1919. The completed results from the study of 4 large concave gratings are presented in the present paper.

2. TYPES OF SECONDARY SPECTRA

The secondary spectra or ghosts produced by diffraction gratings with periodic errors in ruling are of two kinds. Those first observed by Quincke[5] in 1872 are now commonly known as Rowland ghosts. They occur in pairs symmetrically placed on opposite sides of the true spectral line and relatively near this parent line. Their symmetrical positions and proximity to the real line make their recognition easy in simple spectra, but in complicated spectra they may be interspersed among other real lines and often coincide approximately with some of the latter thus leading to erroneous results for the position and intensity of the true spectral lines. The separations of the Rowland ghosts from the parent lines are readily deduced from the characteristics of the ruling engine since these secondary spectra originate in a secondary grating whose spacing is a function of the pitch of screw of the ruling machine. Thus a grating ruled with 20 000 lines per inch on a machine having a screw of 1/20 inch pitch and 1000 teeth in the wheel which turns the screw, will show periodic errors at intervals of 1/20 inch or 1000 lines. This periodicity really constitutes a second grating superposed upon the first and possessing 1000 times the spacing constant of the first, i.e., a grating with 20 lines per inch is superposed on a grating with 20 000 lines per inch. The first order secondary spectral image will therefore be separated from the primary line by 1/1000 the wave-length of this

[3] Meggers and Kiess, B. S. Bulletin, 14, p. 637; 1918.
[4] Meggers, Kiess and Walters, Pub. Am. Astron. Soc., *4*, p. 101; 1920.
[5] Pogg. Ann., *146*, p. 1; 1872.

line and successive higher orders of ghosts will be separated from each other by the same amount or by $\frac{2\lambda}{1000}$, $\frac{3\lambda}{1000}$, etc., from the primary spectrum line of wave-length λ. Similarly, gratings ruled 7500 lines per inch with a screw of 1/20 inch pitch and skipping every other tooth of 750 in the wheel will produce ghosts at positions $\frac{\lambda}{375}$, $\frac{2\lambda}{375}$, $\frac{3\lambda}{375}$, etc. In the same way that different orders of the primary spectra may differ in intensity, the various orders of these secondary spectra may show wide differences in relative intensity, some being absent or very weak compared to others. The theory of these secondary spectra was discussed by Rowland who deduced that the intensity of the first order ghosts is proportional to the square of the order of the primary spectrum and to the square of the relative deviation from the ruled spacing of the primary grating.

Another type of secondary spectra or spurious lines was first described by Lyman[6] and are now referred to as Lyman ghosts. They occur at wide intervals in the spectrum, at considerable distances from the true lines and are therefore generally difficult to distinguish from real lines in the same spectral region. The false lines to which Lyman called attention were produced by Rowland gratings with 14 438 lines per inch and occurred in pairs near positions in the spectrum corresponding to $\frac{1}{3}$ and $\frac{2}{3}$ the wave-length of true lines. Similar spurious lines have been found at the Bureau of Standards in an investigation of Anderson gratings ruled with 5000 and 7500 lines per inch. In the cases of these gratings the spurious lines appeared in groups, generally quadruplets near positions corresponding to $\frac{2\lambda}{5}$, $\frac{3\lambda}{5}$, $\frac{4\lambda}{5}$, $\frac{6\lambda}{5}$, $\frac{7\lambda}{5}$, $\frac{8\lambda}{5}$, and $\frac{9\lambda}{5}$. The origin of these types of secondary spectra has never been satisfactorily explained. Lyman attempted an explanation, suggested by Runge, to account for lines near $\frac{\lambda}{3}$ and $\frac{2\lambda}{3}$ but it was not definitely shown that the suspected periodic errors of ruling had any relation to the actual errors expected from the method of

[6] Phy. Rev., *12*, p. 1, 1901; *16*, p. 257, 1903.

ruling. Furthermore, the positions of these Lyman ghosts were generally in the extreme ultra violet or Schumann region where suitable wave-length standards were not available and the position or wave-length measurements were therefore not precise enough to test or develop any complete theory.

The investigations conducted at the Bureau of Standards resulted in fairly precise measurements of the apparent wave-lengths of the false spectra and some relations were found among the apparent wave-lengths which it seemed might be traced to certain known properties of the ruling engine used in the manufacture of the diffraction gratings. Some of the diffraction gratings used in this investigation will now be described and the experimental results of the investigation will be given.

3. EXPERIMENTAL RESULTS

Four different diffraction gratings with radii of curvature approximating 21½ feet (640 cm) were investigated for false or secondary spectra of both types, i.e., Rowland and Lyman ghosts. The instruments were set up in parallel light on the same mounting which has been described previously.[7] The results tabulated below were all obtained photographically, various sources, ray filters and photographic plates being used for different spectral regions. Only a summary of the observations is given in this paper. Further details and a complete record of the observational data will appear later in the Bulletin of the Bureau of Standards.

(1) The first grating employed in this investigation was ruled at the Johns Hopkins University by Dr. J. A. Anderson on May 1, 1912, and was tested by him on November 21, 1913. It received the serial number 222 and is described as a "6-inch" grating with a radius of curvature of 640 ± cm. It has a ruled surface of 13.3 × 7.5 cm and has 7500 lines per inch (299 per mm) or a total of 39800 lines. The characteristics of its spectra were given by Dr. Anderson as follows:

Order of Spectrum	0	1*R*	1*L*	2*R*	2*L*	3*R*	3*L*	4*R*	4*L*	5*R*	5*L*
Relative brightness λ5890	4	2	8	3	2	3	4	3	2	2	1
Resolving power % of theoretical		90± throughout									
Ghosts relative intensity		Weak									

[7] B. S. Bulletin, *14*, p. 371; 1917.

The various false spectra actually observed in spectrum investigations with this grating, using the order of spectrum designated as 1L, are summarized in the following table in which λ_g represents the apparent wave-length of a ghost related to the true wave-length λ by certain factors:

(a) Rowland ghosts for grating No. 222, 7500 lines per inch:

$$\lambda_g = (1 \pm n/375)\lambda$$

Relative intensities in primary spectrum of first order:

n =	0	1	2	3	4	5	6
i =	5000	2	1−	1−	2	1−	1−

(b) Lyman ghosts for grating No. 222, 7500 lines per inch:

Group	*Wave-lengths*	*Observed factors*	*Intensity*
$\frac{\lambda}{5}$	Looked for but not found.		0
$\frac{2\lambda}{5}$	$\lambda_g = (2/5-1/1500)\,\lambda$	0.399294	3
	$\lambda_g = (2/5+4/1500)\,\lambda$	0.402640	1
$\frac{3\lambda}{5}$	$\lambda_g = (3/5-4/1500)\,\lambda$	0.597312	1
	$\lambda_g = (3/5+1/1500)\,\lambda$	0.600686	3
	$\lambda_g = (4/5-7/1500)\,\lambda$	0.795307	1−
$\frac{4\lambda}{5}$	$\lambda_g = (4/5-2/1500)\,\lambda$	0.798670	5
	$\lambda_g = (4/5+3/1500)\,\lambda$	0.802002	2
	$\lambda_g = (4/5+8/1500)\,\lambda$	0.805369	1
$\frac{5\lambda}{5}$	Primary spectrum of real lines		100 000
	$\lambda_g = (6/5-8/1500)\,\lambda$	1.194651	1
$\frac{6\lambda}{5}$	$\lambda_g = (6/5-3/1500)\,\lambda$	1.197995	2
	$\lambda_g = (6/5+2/1500)\,\lambda$	1.201355	5
	$\lambda_g = (6/5+7/1500)\,\lambda$	1.204705	1
	$\lambda_g = (7/5-6/1500)\,\lambda$	1.396038	1
$\frac{7\lambda}{5}$	$\lambda_g = (7/5-1/1500)\,\lambda$	1.399367	10
	$\lambda_g = (7/5+4/1500)\,\lambda$	1.402710	3
	$\lambda_g = (7/5+9/1500)\,\lambda$	1.405964	1
	$\lambda_g = (8/5-9/1500)\,\lambda$	1.594082	1
$\frac{8\lambda}{5}$	$\lambda_g = (8/5-4/1500)\,\lambda$	1.597362	2
	$\lambda_g = (8/5+1/1500)\,\lambda$	1.600698	8
	$\lambda_g = (8/5+6/1500)\,\lambda$	1.604038	1
	$\lambda_g = (9/5-7/1500)\,\lambda$	1.795348	1−
$\frac{9\lambda}{5}$	$\lambda_g = (9/5-2/1500)\,\lambda$	1.798680	5
	$\lambda_g = (9/5+3/1500)\,\lambda$	1.802016	2
	$\lambda_g = (9/5+8/1500)\,\lambda$	1.805352	1

The relative intensities of these spurious lines within each group show some relation to their distances from the exact $\frac{n\lambda}{5}$ position. For example, in the $\frac{4\lambda}{5}$ group, $\lambda_g = (\frac{4}{5} - 2/1500)\lambda$ is the strongest ghost, $\lambda_g = (\frac{4}{5} + 3/1500)\lambda$ is second in intensity and the remaining two lines $\lambda_g = (\frac{4}{5} - 7/1500)\lambda$ and $\lambda_g = (\frac{4}{5} + 8/1500)\lambda$ are still fainter. Our methods of observing indicate that the $\frac{7\lambda}{5}$ and $\frac{8\lambda}{5}$ groups are the most intense, $\lambda_g = (7/5 - 1/1500)\lambda$ appearing most frequently in wave-length measurements and $\lambda_g = (8/5 + 1/1500)\lambda$ occurring very often.

The numbers assigned for intensities are estimated from the inverse ratios of exposure times necessary to reproduce all to the same photographic density. It is recognized that these numbers are only rough approximations but they probably represent the relative order of magnitude of the intensities. In general our practice in the preparation of spectral tables has been to assign numbers from 1 to 10 as representative of the relative intensity of spectral lines, the strongest lines being called 10 and those just visible in the measuring microscope are intensity 1. On this basis most of the false lines have intensity values of 1 or 2.

Typical examples of the two types of secondary spectra given by the grating described above are reproduced in Fig. 1.

(2) The second grating which was investigated for secondary spectra was also ruled at the Johns Hopkins University in 1912. It is referred to as No. 156—E-P since we received it in a box bearing that number. Dr. Anderson has informed us that this number belongs to a plane grating and suggests that the concave grating which we describe was placed in the wrong box. Since there is no way of determining the correct number of the grating, we will retain the above questionable number for convenience in referring to a concave grating of the following description. It has 5000 lines per inch on about 6 inches of ruled surface, and the radius of curvature is about 21 feet (640 cm). This grating was ruled on the same machine as No. 222 described above, that is, the ruling screw had 20 threads per inch and 750 teeth in the

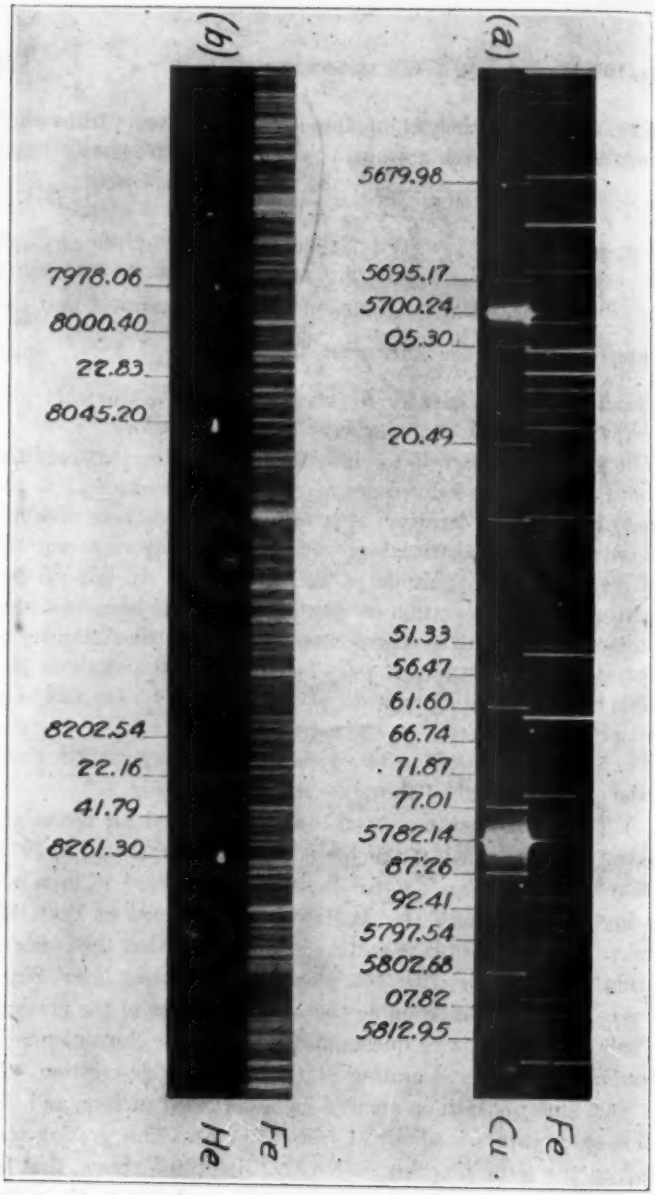

FIG. 1

False spectra: (a) Rowland ghosts, copper arc lines 5700.24 and 5782.14 *A* in 3rd order spectrum of grating *No.* 222. The apparent wave-lengths of the spurious lines may be computed from $\lambda_g = (1 \pm \frac{n}{3 \times 375})\lambda$. It is seen that those for $n=1$ and $n=4$ are much brighter than the others. (b) Lyman ghosts, spurious infra red lines from strong helium lines in the visible spectrum, 1st order of grating *No.* 222. The apparent wave-lengths of the first group are approximately 6/5 of 6678.149 while those of the group at the right are about 7/5 of 5875.618*A*.

screw head of which every other one was omitted in ruling 7500 lines per inch and every third tooth was used in making 5000 lines per inch as in this case.

The Rowland ghosts were investigated in the first six orders of primary spectra and the intensities of these on the scale of 1 to 10 are as follows:

(a) Rowland Ghosts for Grating No. 156—E-P

5000 lines per inch

$$\lambda_n = \left(1 \pm \frac{n}{250m}\right)\lambda, \quad \begin{matrix} n = \text{order of secondary spectrum} \\ m = \text{order of primary spectrum} \end{matrix}$$

Relative intensities	$n=0$	1	2	3	4	5	6	7	8
1st order.....	10	1	0	0	0	0	0	0	0
2nd order.....	10	1	0	0	0	0	0	0	0
3rd order.....	10	1+	0	0	1	0	0	0	0
4th order.....	10	2	0	0	2	1−	0	0	0
5th order.....	10	3	0	0	3	1−	0	0	0
6th order.....	10	3	0	1−	3	1−	1−	0	0

(b) Lyman Ghosts from Grating No. 156—E-P

5000 lines per inch

Group	Wave Lengths	Observed Factors	Intensity
$\frac{\lambda}{5}$	Not found		0
$\frac{2\lambda}{5}$	Not found		0
$\frac{3\lambda}{5}$	$\lambda_n = (3/5 - 4/1750)\,\lambda$	0.597702	1
	$\lambda_n = (3/5 + 1/1750)\,\lambda$	0.600562	3
$\frac{4\lambda}{5}$	$\lambda_n = (4/5 - 2/1750)\,\lambda$	0.798850	2
	$\lambda_n = (4/5 + 3/1750)\,\lambda$	0.801697	1
$\frac{5\lambda}{5}$	Primary spectrum of real lines		100 000
$\frac{6\lambda}{5}$	Not found		0

Group	Wave Lengths	Observed Factors	Intensity
$\frac{7\lambda}{5}$	$\lambda_g=(7/5-1/1750)\,\lambda$	1.399397	1
	$\lambda_g=(7/5+4/1750)\,\lambda$		0
$\frac{8\lambda}{5}$	$\lambda_g=(8/5-4/1750)\,\lambda$	1.597700	2
	$\lambda_g=(8/5+1/1750)\,\lambda$	1.600565	5
$\frac{9\lambda}{5}$	$\lambda_g=(9/5-2/1750)\,\lambda$	1.798781	1
	$\lambda_g=(9/5+3/1750)\,\lambda$	1.801648	2

The first order primary spectra left and right given by this grating were much brighter than the higher orders and both of these first order spectra showed similar secondary spectra of the above type.

(3) In addition to the above described gratings which were ruled on a machine designed to rule a maximum of 15 000 lines per inch, we have examined two gratings which were ruled at the Johns Hopkins University on a different machine, designed to rule a maximum of 20 000 lines per inch. Both were "6 inch" gratings with radius of curvature of about 21 feet (640 ± cm), but one of them bearing the number 330 had 20 000 lines per inch while the other, No. 65–331, had 10 000 lines per inch. Rowland ghosts were observed in several orders of primary spectra for each of these gratings, but neither of them showed any secondary spectra of the Lyman type which we were able to observe. The observations of secondary spectra of the Rowland type are summarized as follows:

(a) Rowland Ghosts for Grating No. 330

20 000 lines per inch.

$$\lambda_g=\left(1\pm\frac{n}{1000m}\right)\lambda$$

Relative intensities in first order primary spectrum:

$n=$	0	1	2	3	4	5	6
$i=10$		1+	0	1−	0	0	0

Relative intensities in second order primary spectrum:

$n=$	0	1	2	3	4	5	6
$i=10$		3	1	2	0	0	0

(b) Rowland Ghosts for Grating No. 65–331
10 000 lines per inch.

$$\lambda_g = \left(1 \pm \frac{n}{500\,m}\right)\lambda$$

Relative intensities in first order primary spectrum:

$n=$	0	1	2	3	4	5	6	7	8	9
$i=$	10	2	1	1−	0	0	0	0	0	0

Relative intensities in second order primary spectrum:

$n=$	0	1	2	3	4	5	6	7	8	9
$i=$	10	4	3−	1.5	1−	1	1−	1	1−	0

Relative intensities in third order primary spectrum:

$n=$	0	1	2	3	4	5	6	7	8	9	10
$i=$	10	5	3	2+	1	2+	1	1+	1	1−	1

4. DISCUSSION

The theory of imperfect gratings has been dealt with by many writers;[8] but these theoretical investigations are confined for the most part to ideal combinations of gratings, emphasize the distribution of intensity in the spectral images, and have little practical value in assisting a worker to detect and eliminate false spectra from his experimental results. No theory thus far advanced explains in a satisfactory manner the possible existence of false lines occurring at considerable distances from the parent. It seemed probable that an experimental study of the false spectra would make it possible in certain cases to indicate just what the constants of the secondary gratings must be and thus give clues as to the origin of the periods which actually occurred in the process of ruling the grating. Our experimental results were transmitted to Prof. Carl Runge, who promptly furnished a complete analysis which explained the Lyman ghosts as due to a

[8] Pierce, Ann. J. Math., *2*, p. 330; 1879.
Rowland, Phil. Mag., (5), *35*, p. 397, 1893; Astronomy and Astro-physics, *12*, p. 129, 1893; Physical Papers, p. 525.
Rayleigh, Coll. Papers, *I*, p. 415; *III*, p. 37.
Cornu, Comptes Rendus, *116*, p. 1215; 1893.
Lyman, Phy. Rev., *16*, p. 257; 1903.
Michelson, Astroph. J., *18*, p. 298; 1903.
Kimbal, Phil. Mag., (6), *6*, p. 30; 1903.
Sparrow, Astroph. J., *49*, p. 65; 1919.

double periodic error. His *Theory of Lyman Ghosts* is given in detail in the paper which follows. In Prof. Runge's theory assumptions are made as to the number of grooves in each period, and both the spacing of the ghosts and their relative intensities are thus accounted for. It was suggested that an examination of the ruling machine might disclose the origin of these periodic errors.

The problem was then transferred to Dr. J. A. Anderson who ruled the gratings in question and who is most intimately acquainted with the mechanical operation and vagaries of this grating ruling machine. His discussion of *Periodic Errors in Ruling Machines* follows the paper by Prof. Runge. We are pleased to express our appreciation for the interest which Prof. Runge and Dr. Anderson took in this problem and we thank them for the valuable assistance which they have given in unravelling the mystery of false spectra from diffraction gratings.

In the interests of precision spectroscopy the writers wish to warn all users of diffraction gratings against the false spectra of different kinds which are no doubt produced by all such instruments. There are only two ways in which spectrum tables are protected from the intrusion of these spurious results. One protection originates in the extremely low intensity of the secondary spectra from some gratings so that careful tests fail to detect their presence, while the other way is to test all observed wavelengths of the less intense lines with ghost factors derived from careful tests of the grating's performance. In either case it is necessary to fall back upon a thorough examination of the grating. This applies especially to spectrum investigations in the infra red and extreme ultra violet where relatively long photographic exposures to very intense sources are required. It may be assumed that secondary spectra of strong lines of the visible spectrum are always present in these out-lying regions and whether or not their intensities are negligible can be determined only by rigid test. For the visible and infra red regions a suitable method of testing for Lyman ghosts is to make a long exposure to an intense monochromatic source like the green line of mercury, the yellow line of helium or the red line of cadmium. The usefulness of a grating

for work in vacuum spectrographs for investigating the very short waves should be tested by making a long exposure with air in the apparatus while the grating is set so as to diffract to the plate a portion of the primary spectrum for which air is opaque. The importance of such tests has not been sufficiently emphasized heretofore and we trust that this discussion of the subject will put spectroscopists on guard against insidious spurious results of the various kinds which may be given by diffraction gratings.

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WASHINGTON, D. C.

PART II. THEORY OF LYMAN GHOSTS

The Lyman ghosts can be explained satisfactorily by the superposition of two periods, a large one and a small one, the former not being a multiple of the latter. They are different from the period corresponding to one revolution of the screw and must arise from some other periodic arrangement in the machine. What this is, an examination of the machine should be able to tell.

For the grating No. 222, described by Meggers and Kiess in Part I, assume the large period of 298 grooves so that the whole grating is composed of a number of smaller ones of 298 grooves. In each of these smaller ones suppose a periodic error repeated every five grooves. The error need not necessarily be an error of spacing, it may as well be a difference in the form of the five grooves. The spectrum of a monochromatic light source of wave length λ produced by one of the smaller gratings will show the direct image of the slit and the lines of the different orders. But besides it will show ghosts corresponding to the period of five grooves.

Let $a\lambda$ be the mean difference of path of two light waves issuing from the light source and reflected from one groove and from the next and observed at a certain place of the spectrum. Then $5a\lambda$ is the difference of path of two resultant light waves reflected from one group of five grooves and the next group. This

group of five grooves is repeated 59 times and we shall have the 59 resultant waves,

$$a\cos\left(\frac{ct}{\lambda}2\pi\right)+a\cos\left(\frac{ct}{\lambda}+5\alpha\right)2\pi+\ldots\ldots+a\cos\left(\frac{ct}{\lambda}+58\times5\alpha\right)2\pi,$$

(in which c = the velocity of light and t = time) and the three waves from the three remaining grooves. I shall assume that these three grooves do not reflect or that the light reflected from them may be neglected.

The amplitude A of the resultant of the 59 waves, as is well known, may be represented in the form

$$A=a\frac{\sin(295\alpha\pi)}{\sin(5\,\alpha\,\pi)}$$

Now let the grating of 298 grooves be repeated n times; the n waves reflected from them and examined at the same point of the spectrum are:

$$A\cos\left(\frac{ct}{\lambda}2\pi\right)+A\cos\left(\frac{ct}{\lambda}+298\,\alpha\right)2\pi+\ldots+A\cos\left(\frac{ct}{\lambda}+(n-1)298\alpha\right)2\pi.$$

The amplitude of this grand resultant is:

$$A\frac{\sin(n\times298\alpha\pi)}{\sin(298\alpha\pi)}=a\frac{\sin(295\times\alpha\pi)}{\sin(5\alpha\pi)}\cdot\frac{\sin(n\times298\alpha\pi)}{\sin(298\alpha\pi)}$$

"a" is also a function of α and depends on the quality of the error in the ruling of the groups of five grooves and on the form of these grooves. But for small variations of α we may treat "a" as a constant. The intensity of the light at the examined point of the spectrum is measured by the square of the amplitude. We shall therefore find a considerable intensity only at those places of the spectrum where neither of the two factors $\frac{\sin(295\alpha\pi)}{\sin(5\alpha\pi)}$ and $\frac{\sin(n\times298\alpha\pi)}{\sin(298\alpha\pi)}$ is very small relatively to its maximum.

The first factor is relatively small except where 5α is an integer or nearly so, and the second factor is small except where 298α is an integer or nearly so. A considerable intensity will therefore only exist at those places where both 5α and 298α are very nearly integers. The deviation of 298α from an integer must be some-

what smaller than $1/n$, for when the deviation is equal to $1/n$ the denominator sin $(n \times a\pi)$ will vanish. The deviation of $5a$ from an integer for a similar reason must be somewhat smaller than $1/59$. Now let n be a number of times larger than 59. Then we may say that $298a$ will have to be equal to an integer m, while $5a$ may deviate from an integer ν by something less than $1/59$, or a from $\nu/5$ by something less than $1/295$. If the period of five grooves be ill-defined the maxima of A^2 will be broader. Say that the zeros of A^2 on both sides of the maxima will be filled up so that considerable values remain on either side about as far as $a = \frac{\nu}{5} \pm \frac{2}{295}$ so that a may deviate to an amount somewhat less than $2/295$ from $\nu/5$ without A^2 becoming very small. We shall then observe lines at those places where $298a = m$, m being an integer for which $m/298$ may deviate from $\nu/5$ to an amount somewhat less than $2/298$, that is to say, an integer between the limits $\frac{\nu 298}{5} - 2$ and $\frac{\nu 298}{5} + 2$. These lines will appear to have the wave lengths $\lambda' = a\lambda$.

In the following table the calculated line ratios $\lambda'/\lambda = m/298$ are compared with the observed line ratios.

The intensity of the line for which the deviation of a from $\nu/5$ is the smallest ought in each group corresponding to the same value of ν to be the strongest, because this place in the spectrum is close to the maximum of A^2. But the intensities of the other lines of the same group need not necessarily be in the order of the deviation of a from $\nu/5$, as the value A^2 may have secondary maxima on both sides of $a = \nu/5$. The groups corresponding to the different values of ν may have different intensities owing to the different values of "a" in different directions.

In the grating mentioned as No. 156—E-P by Meggers and Kiess, the large period contains 348 grooves, while the small period again consists of five grooves. The limits of the integer m can here be reduced to $\frac{\nu}{5} 348 \pm 1$. Table 2 gives the calculated ratios of wave-lengths in comparison with the observed ratios.

TABLE I

ν	$\frac{\nu}{5}298\pm2$	m	$\alpha=m/298$	α observed	obs.−calc.
1	57.6, 61.6	58	0.194631		
		59	0.197987		
		60	0.201342		
		61	0.204699		
2	117.2, 121.2	118	0.395973		
		119	0.399329	0.399294	-35×10^{-6}
		120	0.402684	0.402640	-44×10^{-6}
		121	0.406040		
3	176.8, 180.8	177	0.593960		
		178	0.597315	0.597312	-3×10^{-6}
		179	0.600671	0.600686	$+15\times10^{-6}$
		180	0.604027		
4	236.4, 240.4	237	0.795302	0.795307	$+5\times10^{-6}$
		238	0.798658	0.798670	$+12\times10^{-6}$
		239	0.802013	0.802002	-11×10^{-6}
		240	0.805369	0.805369	○
5	296, 300	297	0.996644	{ principal	
		298	1.000000	{ spectrum	
		299	1.003356		
6	355.6, 359.6	356	1.194631	1.194651	20×10^{-6}
		357	1.197986	1.197995	9×10^{-6}
		358	1.201342	1.201355	13×10^{-6}
		359	1.204698	1.204705	7×10^{-6}
7	415.2, 419.2	416	1.395973	1.396038	65×10^{-6}
		417	1.399329	1.399367	38×10^{-6}
		418	1.402684	1.402710	26×10^{-6}
		419	1.406040	1.405964	-76×10^{-6}
8	474.8, 478.8	475	1.593960	1.594082	122×10^{-6}
		476	1.597315	1.597362	47×10^{-6}
		477	1.600671	1.600698	27×10^{-6}
		478	1.604027	1.604038	11×10^{-6}
9	534.4, 538.4	535	1.795302	1.795348	46×10^{-6}
		536	1.798658	1.798680	22×10^{-6}
		537	1.802013	1.802016	3×10^{-6}
		538	1.805369	1.805352	-17×10^{-6}

TABLE 2

ν	$\frac{\nu}{5}$ 348±1	m	$\alpha = m/348$	α observed	obs.−calc.
1	68.6, 70.6	69	0.198276		
		70	0.201149		
2	138.2, 140.2	139	0.399425		
		140	0.402299		
3	207.8, 209.8	208	0.597701	0.597702	$+ 1\times10^{-6}$
		209	0.600575	0.600562	-13×10^{-6}
4	277.4, 279.4	278	0.798851	0.798850	$- 1\times10^{-6}$
		279	0.801724	0.801697	-27×10^{-6}
5	347 ,349	348	1.000000	principal spectrum	
6	416.6, 418.6	417	1.198276		
		418	1.201149		
7	486.2, 488.2	487	1.399425	1.399397	-28×10^{-6}
		488	1.402299		
8	555.8, 557.8	556	1.597701	1.597700	$- 1\times10^{-6}$
		557	1.600575	1.600565	-10×10^{-6}
9	625.4, 627.4	626	1.798851	1.798781	-70×10^{-6}
		627	1.801724	1.801648	-76×10^{-6}

This is the same explanation that I gave to Mr. Lyman in 1902. The plate he sent me showed his grating to have a large period of 73 grooves and a small period of three grooves.

Notice that in the three cases:

Grating	Large period	Small period	1 Revol. of screw	Number of teeth of screw
Grating No. 222	298 grooves	5 grooves	375 lines	750
Grating No. 156	348 "	5 "	250 "	750
Lyman's Grating	73 "	3 "	720 "	720

the large period contains two grooves less than a round number, $298+2=300$, $348+2=350$, $73+2=75$. These round numbers,

300, 350, and 75, are each exact multiples of the associated small period and may have something to do with the mechanism causing the periods.

UNIVERSITY OF GÖTTINGEN

PART III. PERIODIC ERRORS IN RULING MACHINES

1. GENERAL CONSIDERATIONS

For simplicity consider a machine like that of Professor Rowland, good descriptions of which may be found in Baly's Spectroscopy, in Kayser's Handbuch der Spektroscopie, or in *Physical Papers of Henry A. Rowland.*

Since one line of the grating is ruled for each complete revolution of the drive shaft, it will be convenient to call such a revolution a cycle. The number of teeth on the spacing wheel will be denoted by N, the pitch of the screw by P, and the number of teeth spaced per cycle K, where K in general is equal to unity, but may be any other small whole number.

In each cycle the screw turns through an angle $\frac{2\pi K}{N}$, the nut which moves the grating carriage, advances parallel to the axis of the screw and with reference to a fixed point on this axis by the amount $\frac{KP}{N}$; and if the screw does not move in a direction parallel to its own axis with reference to the frame of the machine, the grating carriage will advance along its way by the same amount $\frac{KP}{N}$. This quantity is the grating space and it will be denoted by a. In an ideal grating a must be constant, but in practice this is never the case. It will therefore be necessary to consider how much a may vary without interfering seriously with the quality of the grating. For this purpose let the distance of any line n from one edge of the grating, that is from the line $n=0$ be denoted by x_n. In the ideal grating

$$x_n = a\,n \tag{1}$$

While in actual grating

$$x'_n = a\,n \pm e_n \tag{2}$$

There are two important special cases, which in any actual grating will be found superposed, but which may be considered separately:

(1) The values of e_n for different values of n are distributed at random. Such errors may be spoken of as accidental, and it will be observed that the distance from one line to the next line will differ from a by a quantity equal to $e_r - e_{r+1}$, which, since plus and minus signs of e_n are equally probable will be often simply the sum of the two e's. Now, according to a general theorem, a wave-front is not seriously impaired by accidental variations in phase provided these are smaller than $\pi/2$ or $\frac{1}{4}$ of a wave-length. For a reflection from a surface at normal incidence, this demands that the errors in the surface should not be greater than $\frac{1}{8}$ of a wave-length and in the case of a grating it will be seen that

$$e_r + e_{r+1} < \lambda/8 \text{ or,}$$

$$e_n < \lambda/16 = \text{roughly } \frac{1}{1\,000\,000} \text{ inch or } \frac{1}{400\,000} \text{ cm} \qquad (3)$$

If accidental errors much greater than this are present, the quality of the grating will surely be impaired. It will be observed that as far as accidental errors are concerned the grating space may vary as much as e_n in equation (3) without doing any great harm.

(2) The values of e_n are periodic with a period N and may be expressed as a Fourier series of the form

$$e_n = b \sin \frac{2\pi n}{N} + c \sin \frac{4\pi n}{N} + d \sin \frac{6\pi n}{N} + \ldots\ldots\ldots\ldots (4)$$

As is well known, this type of error in a grating gives rise to ghosts easily observable in a bright line spectrum. Due to the term $b \sin \frac{2\pi n}{N}$, a line of wave length λ will be accompanied by two ghosts which in the spectrum of order s will occupy the position $\lambda\left(1 \pm \frac{1}{sN}\right)$ and whose intensity is given by

$$I_g = I_0 \frac{s^2}{4} \left(\frac{2\pi b}{a}\right)^2 \qquad (5)$$

provided $b < \frac{a}{2\pi}$, I_0 being the intensity of the line λ. Similarly the term $c \sin \frac{4\pi n}{N}$ will give ghosts in the positions $\lambda\left(1 \pm \frac{2}{sN}\right)$ and whose intensity is $I_g = I_0 \frac{s^2}{4}\left(\frac{2\pi c}{4}\right)^2$ etc.

As it is desirable that the ghosts be not too strong, the condition may be imposed that in the first order $1000\, I_g = I_0$, which substituted in (5) gives

$$b = \frac{a}{99.4} = \frac{a}{100} \text{ approximately.}$$

Evidently the same value holds for c, d, etc. Since by formula (5) the intensity of the ghosts varies as the square of the order, the intensity in the fifth order spectrum is 1/40 of that of the primary lines. If the periodic error be doubled, the intensity of the ghosts becomes four times as great, in which case they would begin to be troublesome, especially in the higher orders of spectra. Hence, b, c, d, etc., should not be much greater than $a/100$. Since this is also the value of e_n in equation 2, it follows that, taking $a = \frac{1}{15\,000}$ inch (or $\frac{1}{6000}$ cm) the value of e_n comes out of the same order of magnitude as that given above the equation 3. In regard to the constancy of a, however, the case under discussion differs from that considered above. The greatest value of a will be given by $a + \left(\frac{de_n}{dn}\right)_{max}$ or, considering only the first term on the right side of equation (4,)

$$a + (\Delta a)_{max.} = a + \left(\frac{de_n}{dn}\right)_{max.} = a + \frac{2\pi}{N} b \qquad (6)$$

In Rowland's 15,000 machine, $N = 750$ and hence

$$(\Delta a)_{max.} = \frac{2\pi}{750} b = \frac{a}{12\,000} \text{ approximately.}$$

Corresponding to the terms $c \sin \frac{4\pi n}{N}$, $d \sin \frac{6\pi n}{N}$, etc., the values of $(\Delta a)_{max.}$ would be $\frac{2a}{12\,000}$, $\frac{3a}{12\,000}$ etc., respectively.

Ideal Rigid Machine. If all parts of the machine be regarded as perfectly rigid, then from purely geometrical considerations it is a simple matter to calculate how accurately the various parts must be constructed in order that the spacing errors shall be within the limits just derived. The only parts that need to be discussed are those that are involved in the motion of the grating carriage, for obviously no periodic errors are likely to be produced by the ruling carriage since it completes a cycle in the ruling of each line. These parts are: the spacing wheel; the thrust bearing; the pivots of the screw; the connecting mechanism between the nut and grating carriage; and the screw itself with its nut.

1. *The Spacing Wheel.* Let the radius of the wheel be r and the distance between two teeth on its circumference be s. Then if the spacing constant K is unity, the grating space a is given by

$$a = \frac{s}{2\pi r} P$$

Regarding s and r in turn as variables,

$$\Delta s = \frac{2\pi r}{P} \Delta a \text{ and } \Delta r = -\frac{2\pi r^2}{sP} \Delta a.$$

In Rowland's 15 000 machine $r = 7$ in, and $P = 1/20$ inch. Taking $a = \frac{1}{1\ 000\ 000}$ inch, the value found for allowable accidental errors, the value $\Delta s = \frac{1}{1140}$ inch will make these errors sufficiently small, whereas there is no difficulty in making Δs as small as $\frac{1}{10\ 000}$ inch in practice. Periodic errors are likely to be introduced by an eccentricity in the wheel and this is measured by Δr. Taking $\Delta a = \frac{1}{180\ 000\ 000}$ inch, $\Delta r = \frac{1}{1750}$ inch. There is no difficulty in centering the wheel so accurately that the error will be only a fraction of this allowable amount. There remains then only periodic errors in the spacing of the teeth on the circumference of the wheel itself. Since a expressed in angular measure is 1728 seconds of arc, which corresponds to the grating space a, and since a periodic error of amplitude $\frac{a}{100}$ is relatively harmless it is

seen that periodic errors having an amplitude of 17.28 seconds are of little importance. There is no difficulty in reducing these errors to about 1/10 of this amount. Hence the conclusion that the spacing wheel presents no practical difficulties, is obvious.

2. *The Thrust Bearing.* As the screw rotates it will also move parallel to its own axis, due to imperfections in the thrust bearing. This motion is necessarily periodic, the period being one rotation of the screw. The amplitude of this motion must be made less than $\frac{a}{100}$ and this can be accomplished as follows: A plate of some hard material like ruby is worked and polished optically plane. It is mounted on the end of the screw so that its normal is as nearly as possible parallel to the screw axis. If the diameter of the plate is not less than ¼ inch this can be done to well within 2 seconds of arc. A convex steel surface rigidly mounted on the frame of the machine makes contact with this plate as nearly on the axis of rotation as possible. This coincidence will not be exact, but a departure of much more than 1/10 mm can easily be avoided. The amplitude of the motion parallel to the axis of the screw is then $1/10 \text{ mm} \times \sin 2'' = 10^{-7}$ cm, about, or about 1/40 of the allowable amount.

3. *The Pivots of the Screw.* If the axis of the pivots and the axis of the screw do not coincide, the nut will move in a spiral path instead of in a straight line, and since the axes can never be made to coincide exactly, the path is always a spiral. It is desirable to make the radius of this spiral as small as possible. This may be accomplished as follows: when the screw has been ground and polished it is mounted so that it can be rotated about the axis of its pivots. A well-fitting nut is made to carry one plate of an interferometer so adjusted that its reflecting face is parallel to the axis of rotation, the other plate being mounted on the frame supporting the bearings. The interference fringes are observed as the screw is rotated. By correcting the pivots until no motion of the interference fringes is observable, it is possible to reduce the radius of the spiral to a small fraction of a wave-length of light.

4. *The Connecting Mechanism between the Nut and Grating Carriage.* Since the spiral motion of the nut just discussed

cannot be entirely avoided and since it is impossible to have the axis of the screw *exactly* parallel to the grating-carriage way, it follows that a rigid connection between the nut and carriage can not be considered. In principle, the connection consists of two flat plates on the nut making contact with corresponding convex surfaces on the carriage. The plane of the plates is as nearly normal to the axis of rotation of the screw as possible. Since the radius of the spiral described by the nut is so very small, it follows that no appreciable periodic error is introduced here, assuming, of course, that all parts are perfectly rigid. (In an actual machine, where the parts are elastic and it becomes necessary to take friction into account the case is very different, as will appear later.)

5. *The Screw and Nut.* Only two points need to be considered: The axis of the screw must be straight, and its effective diameter must be constant throughout the length actually used. Only the first of these is of any importance for the case of periodic errors, since variations in the diameter of the screw can only introduce slow variations in the effective pitch.

In general if a screw is cut from a piece of well-annealed tool steel and is ground and polished by a long nut, its axis will be so nearly straight that the most refined interferometer method will fail to reveal any curvature. The sensitivity of the interferometer test may be deduced as follows: Let two nuts be mounted on the screw with their centers a distance l apart; one interferometer plate is mounted on each nut, and at a distance d above the axis of the screw. Let R be the radius of curvature of the screw axis, and let z be the maximum displacement of one plate with reference to the other in one rotation of the screw.

$$\text{Then} \quad z=\frac{2dl}{R} \quad \text{or} \quad R=\frac{2dl}{z} \tag{8}$$

Taking $z=\dfrac{1}{500\,000}$ inch as easily observable, and $d=l=6$ in., it follows that $R=36\times10^6$ inches or about 600 miles. A slight curvature of the screw axis, if present, will produce a variable spiral motion of the nut with reference to the grating carriage, but no periodic error in its motion *along* the axis of the screw.

(In the case of a screw and nut which have been fitted to each other by grinding and polishing, there can, of course, be no question of any periodic error in the screw threads themselves. If, indeed, any such error be present, it must be of a very different order of small quantities from those considered here.)

Given perfectly rigid materials, it is hence reasonable to say that a machine could very easily be constructed which would space lines with accidental errors less than 1/100 of a wave length, and with no periodic errors as large as 1/500 of the grating space (assumed as 1/15 000 inch). The intensity of the ghosts in, say, the fourth order of a grating of this accuracy would be about 1/1600 of that of the spectrum line itself.

3. ACTUAL MACHINE. EFFECTS OF FRICTION

Since all materials are deformable it is necessary to take into account the effects produced by forces due to friction, and as a result the conclusions stated above do not apply to an actual ruling machine. It is generally found that a machine which has been built and adjusted so carefully that both accidental and periodic errors ought to be less than 1/10 of the allowable magnitude will, when tried, give errors so large that gratings ruled on it are practically of no value. To account for this is not difficult. Let the force required to move the ruling carriage along its ways after ruling has been in progress for a few hours be F. The time condition is important, for the force required to move the carriage increases rapidly at first becoming constant only after several hours of operation. The force F may be taken as a measure of the elastic deformation of the parts lying between the thrust bearing and the area of contact between the grating carriage and its way. Beginning at the thrust bearing, these are, the part of the screw between the nut and the thrust plate, the oil-film between the threads of the screw and nut, the nut itself, the connection between the nut and carriage, and the parts of the carriage. Given the value of F, the deformation of some of the parts can be computed; for example, if $F=40$ lbs, as is usual with Rowland's 15 000 machine, the compression of the screw when the nut is at its middle is about one half a wave-length of light. The deformation of the connecting mechanism between the nut and carriage is consider-

ably greater. The change in the oil-film is perhaps the most important and there is some evidence that this shows an appreciable lag. The sum of the deformations of all the parts was measured at various times, and was found to lie between five and ten wavelengths, depending upon the load on the carriage. If this quantity be denoted by ΔX, then by Hooke's law,

$$\Delta X = h F \tag{9}$$

where h is a quantity depending upon the nature of the parts just mentioned, being a constant or nearly so for a given machine in a given state of adjustment.

Since ΔX is of the order of $2a$, and must remain constant within $a/100$, it follows that during the ruling of any one grating F must remain constant to about $1/200$ of its value. Referring to paragraphs 3 and 4, section II, it will now be clear that a very small spiral motion of the nut, due to slight errors in the screw pivots, may produce considerable periodic errors of spacing. For such a spiral motion necessarily introduces a periodic variation in the value of F in equation (9). Perhaps it is fair to say that an important part of the problem of ruling good gratings is the problem of keeping F constant within a few tenths of one per cent of its value.

4. PROBABLE ORIGIN OF LYMAN GHOSTS

Professor Runge has shown that Lyman Ghosts may be accounted for analytically by assuming two periods, both of which in general are shorter than the fundamental period N of the machine. The same analysis would perhaps apply if the shorter of the two periods be assumed to change its shape in a periodic manner, thus introducing the second period required. The periods which have been found experimentally up to date are given in Table 1.

TABLE 1

No.	Observer	Grating Space	Size of Grating	Machine	Periods
1	Lyman	14 438	Concave 1 meter	14 438	3 and 73
2	Meggers & Kiess	7 500	" 21 feet	15 000	5 and 298
3	Meggers & Kiess	5 000	" 21 feet	15 000	5 and 348
4	Anderson	15 000	" 15 feet	15 000	5 and 277
5	Anderson	15 000	" 1 meter	15 000	5 and 277
6	Anderson	15 000	" 1 meter	15 000	5 and 320+

Observations Nos. 4, 5 and 6 have been made recently, using three gratings belonging to the Mount Wilson Observatory. The gratings used in observations 2 to 6 were all ruled by the writer on Rowland's 15 000 machine, some time between Dec. 1911 and August 1912. It will be observed that the period 5 is common to all of these, but that the longer period is variable.

Observations Nos. 5 and 6 were made in such a way that a fair estimate of the intensity of the Lyman Ghosts in terms of that of the line giving rise to them could be made. With both of these gratings the intensity of the strongest Lyman Ghost is of the order of $\frac{1}{50\,000}$ of that of the line itself. While formula (5) may not apply exactly to this case, it will at least give the order of magnitude of the amplitude b which, by placing $s=1$, gives $b=\frac{a}{700}$. To produce this error, F, in equation (9), would have to vary in a period of 5 lines by only $\frac{1}{1400}$ of its value. Now, it happens that the driving belt of the 15 000 machine makes 2 revolutions for each 5+cycles of the machine. The belt exerts an upward pull on the machine which will vary somewhat if the belt is not uniform in thickness, owing to the fact that the driving pulley is small, being only two inches in diameter. This varying pull with its resultant distortion of the frame of the machine may perhaps be sufficient to cause the required change in F. If so the period 5 and its constancy is accounted for. Slight variations in the length of the belt due to stretching with use might explain the variations in the length of the longer period.

The following quotation from a paper by Henry A. Rowland is of interest in this connection. (Physical Papers, page 536):

"So sensitive is a dividing engine to periodic disturbance that all the belts driving the machine must never revolve in periods containing an aliquot number of lines of the grating; otherwise they are sure to make spectra due to their period."

MT. WILSON OBSERVATORY

ON THE PROPAGATION OF LIGHT IN ROTATING SYSTEMS, A REJOINDER TO DR. A. C. LUNN

By
Ludwik Silberstein

Dr. A. C. Lunn in his comments[1] on my paper on this subject[2] proposes to deal "chiefly" with my main contention, but before doing so points out at some length a number of "minor items."

That contention was that a fractional shift effect as a possible outcome of the discussed terrestrial optical experiment, now conducted by Prof. Michelson, would be crucial, namely against Einstein's relativity theory *such as it is*[3] (*i.e.* with the express inclusion of $ds = o$ as the law of light propagation and of $\delta \int ds = o$, with the same ds, as the law of free motion), and favourable to the revival of an aether sharing in part the earth's rotation. Since Dr. Lunn has in the meantime admitted the essential correctness of this result, in a conversation at the recent Lorentz Colloqium at Madison,[4] I need not insist here any more upon it. But the "minor items" are of such a nature as to call for an explicit reply.

In the first place then, Dr. Lunn, while granting readily the claimed necessity of a reference frame for rotation "in spite of appearances to the contrary" (5, p. 291), wonders what those appearances are. Now, such a clause (made, moreover, rather incidentally) has at the time of writing seemed worth making in view of the usual presentation,—even in such fine books as Poincare's 'Science and Hypothesis,'—which is apt to leave the average reader under the impression that it has after all a sense to

[1] This Journal, *6*, p. 112-120; March 1922.

[2] *Ibidem*, *5*, p. 291-307; July, 1921.

[3] It is manifestly impossible to assert anything against it as it might be, *i. e.*, against a modification of Einstein's theory which Dr. Lunn may have vaguely in mind but which he does not specify.

[4] University of Wisconsin, where the subject was brought up by the present writer on March 30 in a paper entitled "The rotating earth as a reference system for light propagation," its conclusions being fully adhered to by Prof. H. A. Lorentz.

speak of the rotation, say, of a perpetually clouded earth, without reference to some assignable framework, such as that of the fixed stars. (See, *e.g.*, p. 141 of Poincare's book, French edition.) But is it really necessary to say any more in justification of a few words of warning inserted in my paper against a possible misconception?

Secondly, in saying that the relativity theory proved unable to deduce the terrestrial *ds* as a gravitational effect, my intention was not to emphasise it as a "flaw in that theory," as Dr. Lunn thinks, but simply to refer to it as a matter of fact. I may be permitted perhaps to mention that, though not a fanatical relativist, I am the last man to be blind to the boldness and beauty of Einstein's theory, and certainly not hostile or prejudiced against it. As to my calling Thirring's solution, in this connection, a "complete failure" (p. 304), though, as I added, mathematically interesting, I do not share Dr. Lunn's impression that I have been unjust to Thirring. It is true that his solution is the result of an avowedly approximate method only, and in view of this one would certainly have to be lenient to some numerical discrepancies. But if these go so far as to yield for the numerical factor of the Coriolis force, as compared with that of the centrifugal one, the value *eight* instead of *two*, and more recently even *ten* instead of *two*,[5] the solution is no more an approximation but simply a misrepresentation of the experimental facts, even if (as I did) one closes his eyes to the superfluous longitudinal force twice as large as the transversal or proper centrifugal force. And the failure seems "complete" indeed when one remembers how simply the correct formula for those experimental facts follows on the classical kinematics. Thirring's hollow spherical shell, rotating around our planet, is certainly not known to represent anything approaching the actual distribution of celestial matter. Yet it seems very doubtful whether anything short of a homogeneous distribution of matter throughout the whole space, which, moreover, has to

[5] In Thirring's original paper of 1918 the factors of the two "forces" (accelerations) were 8/3 and 1/3, but after the amendment of an arithmetical error (Phys. Zeitschrift *22*, p. 29, 1921,) they turned out to be 8/3 and 4/15, bearing to each other the ratio 10 instead of 2.

be assumed to be closed (elliptic), can essentially improve that solution. As I gather from a conversation with Einstein, this would, in his opinion, be the only possible way out. Now, although there are no serious objections to a finite, closed space, as first proposed in his 'Cosmological Contemplations' of 1917, the assumption of a homogeneous distribution of matter throughout the universe is very hard to adhere to. In fact, although Einstein's sensational formula, total mass of the universe equal to $\frac{\pi c^2}{4}$ times the curvature radius of space,[6] seems to be compatible with as small an average density as we like, yet the required homogeneity of its distribution could hold only on such a gigantically macroscopic scale for which the 'volume-element' would be a cube whose sides are ½ to 10 million light years long, this being the order of the mutual distances of Shapley's island universes. Now, such a coarse homogeneity would suffice for the purpose in hand only if the number of those "island universes" themselves would still be enormous, which—for the present at least—is entirely beyond our knowledge. In fine, while Dr. Lunn sees here but a passing difficulty, which he compares with the (Newtonian) retouching of the errors of some early results of celestial mechanics, the present writer is impressed by the gravitational aspect of rotation as a very hard and perhaps unsolvable problem.

Thirdly, concerning the field of competency of special relativity, I must insist most decidedly upon what was said on page 302 of my paper. It is admitted on all hands that Einstein's older or restricted relativity theory, though it can and does consider any non-uniform motions of a particle within any of its privileged, *i.e.* inertial systems, yet does not as a matter of fact deal with any frameworks other than the inertial ones as *reference-systems*, nor has it ever proposed to deal with them. In fact, not a single one of the host of papers, pamphlets and text-books written on that subject deals with any but the inertial reference frames and, correspondingly, with any but the Lorentz transformation as the bridge from one to another such system. So much so that the last

[6] See, for instance, the writer's *General Relativity and Gravitation*, Univ. of Toronto Press, p. 134, 1922.

edition (1919) of Laue's excellent book on special relativity has been entitled by him expressly the "Relativity principle of the Lorentz transformation." But it is not merely the sanction of the said restriction by general usage that supports my thesis. Einstein's older theory is by its very structure the geometry of a metrical four-fold determined by the line-element $ds^2 = c^2dt^2 - dx^2 - dy^2 - dz^2$, and has, in harmony with this, all of its material (four-vectors, six-vectors, *lor* and other derived operators) defined in relation to the Lorentz transformations. The latter form a group, and the whole field of this group is exhausted by the privileged class of inertial systems and *vice versa*, leaving no place for other reference systems.

Next, concerning the rule of convexity of light rays, "clockwise" in the footnote on page 295 is a manifest misprint for "anticlockwise," as Dr. Lunn could readily see from Fig. 2 and Fig. 4 where the arrows indicating the rotation are drawn in the correct sense.[7]

Further, with regard to the diagram on page 300 illustrative of the optical circuit, the three pairs of (curved) rays were drawn only for the sake of simplicity between the same points A, B, C, but they need not be taken as splits of originally the same ray arriving from the collimator. As agreed upon in a conversation with Prof. Michelson, the ultimate interpretation of his pending experimental results will have to be based upon a careful tracing of rays or waves through the whole apparatus with due attention, of course, to the finite breadth of the light beam. But it will be time to undertake such a tracing, laborious though offering no essential difficulties, when the effect will, probably next summer, be measured by Michelson. For a first orientation the said diagram has seemed most appropriate, especially as it brings out the essential compensation of the effects of ray curvature upon the ultimate phase difference or shift effect.

Finally, that r on page 303 stands for a cylindrical coordinate, and that this also is referred to in the footnote on p. 306, is really too obvious to call for so many words.

[7] Another shocking misprint, not noticed by Dr. Lunn, occurred on p. 292, where 10^{-5} in the value of $\bar{\omega}/c$ should read 10^{-15}. A few other misprints, attributable to a sudden change of the Press at that epoch, are too obvious to need a special mention.

It may still be mentioned, in reply to Dr. Lunn's last paragraph of p. 117, that speaking of the possibilities of a revived aether in the case of a fractional shift effect I had in mind a *non-rigid* aether, as will appear most clearly from page 292, where it is said that the spinning drag of the aether may vary from point to point.

ROCHESTER, N. Y.,
May 21, 1922

ON THE OPTICAL CONSTANTS OF SELENIUM IN THE FORM OF ISOLATED CRYSTALS

BY
L. P. SIEG

I. INTRODUCTORY

The optical constants of absorbing materials are of considerable interest in view of the information they may contribute toward the solution of the problem of the structure of matter. The optical constants usually referred to are the index of refraction, the coefficient of absorption, and, derived from these, the reflecting power. Many substances have been studied by various observers, and long lists of these constants are to be found in our reference tables. In view of the fact, however, that the crystalline state of matter is the normal and natural state, it would appear that more attention should be centered upon obtaining these constants, as far as possible, for single crystals of the substances. This matter becomes significant if one cites, for example, the metal zinc. The fundamental crystal form of this element is hexagonal, and as such we should certainly not expect it to possess merely one set of optical constants. In this case there should be two sets, and these should differ from each other in some manner not to be predicted without intimate study of an isolated crystal. And yet our tables contain data for the optical constants of zinc, in which only one set is assumed and recorded. This result is of course to be expected when we consider that the usual practice is to take a slab of the material in question, containing countless crystals, oriented in various ways, polish the specimen, and proceed to determine the constants. The results thus obtained can mean little unless perhaps they represent some intermediate value of the true constants.

As a rule the index of refraction and the coefficient of absorption for highly absorbing substances must be determined by some indirect means, because the substances are too dense to transmit

sufficient light for direct measurements. The regular procedure is to reflect plane polarized light, usually at an azimuth of 45° from the surface in question. By virtue of the absorption and conduction possessed by the medium there is a phase difference introduced into the reflected components of the polarized light. From a knowledge of the form of the reflected elliptically polarized light one can calculate the optical constants by well-known formulas.

It was some years ago, with these ideas in mind, that I set Mr. C. H. Skinner[1] the problem of determining the optical constants of a single crystal of selenium. Skinner followed the regular Babinet compensator method, and found, as was expected, that different results were obtained with a given azimuth and incidence, depending upon whether the long axis of the hexagonal crystal was perpendicular or parallel to the plane of incidence. In such a case the ordinary formulas do not serve, but in their stead we must employ some such expressions as those developed by Drude[2] for absorbing crystalline bodies. In fact Drude,[3] and later Müller,[4] tested these expressions for a natural crystal of antimony sulphide. As far as I am aware, however, this method, with the exception of Skinner's work, has not been applied to elementary substances.

The difficulties which Skinner had in employing so small surfaces, led me to the necessity of seeking some better apparatus for this type of work. Dr. L. D. Weld[5] attacked the problem, and succeeded in devising and constructing an elegant apparatus for the work in question. While his chief problem was the development of the apparatus, he succeeded also in obtaining some data for the ellipticity of the light of various wave lengths, reflected from selenium crystals. His paper contains these data but, at the time of publication, as we were not satisfied that we fully understood Drude's[6] expressions, it was thought best not to reduce

[1] Skinner, Phys. Rev., *9*, p. 148; 1917.
[2] Drude, Ann. d. Phys., *32*, p. 584; 1887.
[3] Drude. Ann. d. Phys., 34, p. 489; 1888.
[4] Müller, Neues Yahrb. für Mineral. u.s.w. *17*, p. 187; 1903.
[5] Weld, J. O. S. A. and R. S. I. *6*, p. 67; 1922.
[6] Loc. cit.

the elliptic constants to optical constants, but to publish them as they stood. Since then I have had some opportunity of examining the equations, and further of determining directly the reflecting powers of these crystals. The experimental results, which will be discussed later in the paper, seem to agree better with Skinner's results than with Weld's.

If one accepts Drude's postulates, one has in the derived equations complete expressions for determining the optical constants for any crystal, uniaxial or biaxial. The difficulties[7] arise in the computations since direct application of the experimental data to Drude's equations yields simultaneous equations between complex quantities of high order. Drude[8] and Müller[9] simplified the equations by certain assumptions as to the relative magnitudes of certain terms. It was really to test the validity of these assumptions that the completion of Weld's work was deferred.

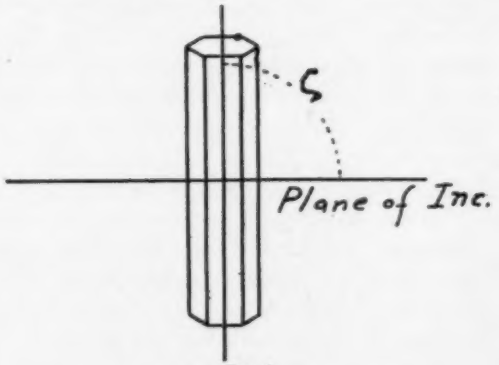

FIG. 1
Orientation of crystal with respect to plane of incidence.

[7] In the Babinet Compensator method as employed by Skinner, no serious difficulties in reducing the results arose, because in that method one adjusts conditions until a phase difference of 90° is introduced. The formulas, for purposes of calculation, are thus vastly simplified. In Weld's apparatus, on the other hand, one obtains a photographic record, and from this calculates the phase differences. It would involve an inordinate labor to repeat observations until a value of 90° for the phase difference were found.

[8] Drude, Ann. d. Phys. *34*, p. 489; 1888.

[9] Müller, Loc. cit.

II. THEORETICAL

One should consult Drude's[10] classic paper for the full theory and the working equations there developed. Here only the few expressions pertinent to this discussion will be considered. In Fig. 1 suppose we have represented one face of the hexagonal crystal lying perpendicular to the plane of incidence, with the optic axis in this case also perpendicular to the plane of incidence. Let ζ be the angle between the optic axis and the plane of incidence. Denote by R_s and R_p the amplitude of the components of the reflected light perpendicular and parallel respectively to the plane of incidence. Then we have

$$r = \frac{R_s}{R_p} e^{i\Delta} = \tan \Psi \cdot e^{i\Delta} \tag{1}$$

where r is a certain useful function of the angle ψ, the azimuth of the reflected elliptically polarized light, and of Δ, the phase difference introduced upon reflection. If we denote by Φ the angle of incidence, and if further one always uses an azimuth of 45° in the incident plane polarized light, so that the incident components perpendicular and parallel respectively to the plane of incidence are equal, one obtains[11]

$$\left.\begin{aligned} r_1 &= \frac{\cos\Phi - \sqrt{\alpha}\,\sqrt{1-\alpha\sin^2\Phi}}{\cos\Phi + \sqrt{\alpha}\,\sqrt{1-\alpha\sin^2\Phi}} \cdot \frac{\sqrt{\gamma}\cos\Phi + \sqrt{1-\gamma\sin^2\Phi}}{\sqrt{\gamma}\cos\Phi - \sqrt{1-\gamma\sin^2\Phi}} \\ r_2 &= \frac{\sqrt{\alpha}\cos\Phi + \sqrt{1-\alpha\sin^2\Phi}}{\sqrt{\alpha}\cos\Phi - \sqrt{1-\alpha\sin^2\Phi}} \cdot \frac{\cos\Phi - \sqrt{\gamma}\,\sqrt{1-\alpha\sin^2\Phi}}{\cos\Phi + \sqrt{\gamma}\,\sqrt{1-\alpha\sin^2\Phi}} \end{aligned}\right\} \tag{2}$$

where r_1 and r_2 refer respectively to the first and second principal positions of the crystal, i.e., where the optic axis is in the surface of the crystal in both cases, but in the first it is perpendicular to ($\zeta = 90°$), and in the latter it is parallel to ($\zeta = 0°$) the plane of incidence. Hereafter the subscripts 1 and 2 shall refer all quantities respectively to these two principal planes. In the above equations α and γ are complex quantities from which one can readily deduce the ordinary optical constants.

[10] Loc. cit. 1887.

[11] Drude, loc. cit. pp. 618-620; 1887.

It is shown by Drude that the following equations (3) are

$$\left.\begin{aligned} R_1 &= \frac{1+r_1}{1-r_1} = \frac{\cos 2\Psi_1}{1-\sin 2\Psi,\cos\Delta_1} + i\,\frac{\sin 2\Psi_1 \sin\Delta_1}{1-\sin 2\Psi,\cos\Delta_1} \\ R_2 &= \frac{1+r_2}{1-r_2} = \frac{\cos 2\Psi_2}{1-\sin 2\Psi_2\cos\Delta_2} + i\,\frac{\sin 2\Psi_2 \sin\Delta_2}{1-\sin 2\Psi_2\cos\Delta_2} \end{aligned}\right\} \quad (3)$$

the final working equations for reducing the elliptical constants Ψ and Δ, for a given incidence Φ, to expressions which lead directly to the optical constants. In attempting to solve rigorously the simultaneous equations (3) one meets some difficulties. As Weld points out in his paper,[12] the suggestion was made that if one should take two different angles of incidence one could reduce the order of equations (3) to the second. Weld had, for one wavelength, used incidences both of 60° and of 45°. However, in attempting to employ these data in calculation it soon became evident that the data for 45° were useless, as they by chance yielded certain phase differences which made the errors involved too large for the method to be of service. I then tried the plan of expanding the equations (3) up to and including terms of the third degree, and a satisfactory solution was found. For simplification in equations (2) let us make the following substitutions:

$$\sin^2\Phi = A^2,\ \cos^2\Phi = B^2,\ \sqrt{\alpha} = x,\ \sqrt{\gamma} = y$$

Remembering that $R_1 = \dfrac{1+r_1}{1-r_1}$, and $R_2 = \dfrac{1+r_2}{1-r_2}$, we have after some reductions and simplifications,

$$\left.\begin{aligned} R_1 &= \frac{B^2y - x\sqrt{1-A^2x^2}\,\sqrt{1-A^2y^2}}{Bxy\sqrt{1-x^2A^2} - B\sqrt{1-A^2y^2}} \\ R_2 &= \frac{B^2x - y(1-A^2x^2)}{(xy-1)B\sqrt{1-A^2x^2}} \end{aligned}\right\} \quad (4)$$

Expanding, and assuming tentatively that both x and y are less than unity, we have

$$\sqrt{1-A^2x^2} = 1 - \frac{A^2x^2}{2} - \ldots . \quad (5)$$

[12] Weld, loc. cit. p. 90.

together with a similar expression for $\sqrt{1-A^2y^2}$. Putting (5) in (4) we have, carrying only to terms of the third degree,

$$\left.\begin{aligned} R_1 &= -By+\frac{x}{B}-\frac{A^2}{2B}x^3-\frac{A^2B}{2}y^3+\frac{x^2y}{B}-Bxy^2+\ldots. \\ R_2 &= -Bx+\frac{y}{B}-\frac{BA^2}{2}x^3+\frac{xy^2}{B}-\frac{A^2+2B^2}{2B^2}\cdot x^2y+\ldots. \end{aligned}\right\} \quad (6)$$

No second degree terms appear in (6). While Drude,[13] and later Müller,[14] in their applications of the general expressions to obtain the optical constants of antimony sulphide did not expand the equations in the above manner, the expressions they used were exactly what one would obtain by retaining only the first two terms on the right hand sides of equations (6). I have adopted their plan in reducing Weld's data, the results of which appear later in this paper. I have, however, for two wave-lengths employed equations (6) as they stand. The resulting optical constants so obtained are not very seriously different from those obtained by using the simpler expressions, but still they differ enough to cause one to have some doubt as to the accuracy claimed by both Drude and Müller. The method of approximating to the solution of equations (6) is in general similar to Newton's method of approximation, excepting that here we are dealing with quantities x and y which are complex. In view of all the pitfalls that ordinarily might appear in applying the following method to the complex plane, I feel that it was only by a fortunate chance that any results came at all.

To make clear the method of computation used, actual calculations from data for $\lambda = 0.55\ \mu$, from Weld's[15] paper, will be outlined. Weld[16] gives for this wave-length, where $\Phi = 60°$, the following:

$$\Delta_1 = \pi + 20°41' \qquad \Psi_1 = 24°26'$$
$$\Delta_2 = \pi + 4°\ 26' \qquad \Psi_2 = 32°27'$$

[13] Loc. cit. 1888.

[14] Loc. cit.

[15] Loc. cit.

[16] Weld gives Δ, as 20° 41', etc., but if the light is viewed as it comes toward an observer, his expression ∇, or $\pi + 20°\ 41'$ must be employed. Drude's equations are developed on this basis.

Applying these data to equations (3) we have

$$\left.\begin{aligned} R_1 &= 0.386 - 0.156\,i \\ R_2 &= 0.223 - 0.037\,i \end{aligned}\right\} \tag{7}$$

Equations (6) become

$$\left.\begin{aligned} .386 - .156\,i &= 2x - y/2 - 3/4\,x^3 - 3/16\,y^3 - xy^2/2 + 2x^2y \ldots \\ .223 - .037\,i &= -x/2 + 2y - 3/16\,x^3 + 2xy^2 - 5/4\,x^2y \ldots \end{aligned}\right\} \tag{8}$$

Employing terms of the first degree only we get the following approximations, using primes to denote approximate values

$$\left.\begin{aligned} x' &= .236 - .088\,i \\ y' &= .170 - .041\,i \end{aligned}\right\} \tag{9}$$

Substituting the values of x' and y' in the terms of (8), which were previously omitted, we have, changing signs

$$\Delta R_1 = -.0038 + .004\,i$$
$$\Delta R_2 = -.0009 - .0026\,i$$

From (8), taking the derivatives

$$-.0038 + .004i = (2 - 9/4\,x^2 - y^2/2 + 4xy)\Delta x + (2x^2 - xy - 9/16y^2 - 1/2)\Delta y$$
$$-.0009 - .0026i = (2y^2 - 5/2\,xy - 9/16\,x^2 - 1/2)\Delta x + (2 + 4xy - 5/4\,x^2)\Delta y$$

or substituting the approximate values x' and y' from (9)

$$-.0038 + .004i = (2.0244 + .0015i)\Delta x + (-.4558 - .0505i)\Delta y$$
$$-.0009 - .0026i = (-.5638 + .0572i)\Delta x + (2.0860 - .0469i)\Delta y$$

whence, solving the above,

$$\Delta x = -.00207 + .00179i,\ \Delta y = -.00092 - .00073i$$

and the next approximations for x and y are

$$\left.\begin{aligned} x &= x' + \Delta x = .234 - .086i \\ y &= y' + \Delta y = .169 - .042i \end{aligned}\right\} \tag{10}$$

No closer approximation is needed, since substitution of the values from (10) in (8) yield the following

$$\left.\begin{aligned} R_1 &= .387 - .155i \\ R_2 &= .222 - .039i \end{aligned}\right\} \tag{11}$$

which are as close to the true values as the errors of experiment warrant.

As simple as the above is in theory, the actual computations, to insure accuracy, is a matter of several hours' work. In view of

this, but especially in view of the very small corrections necessary, this detailed method was used for only one other wave-length, $\lambda = .75\mu$. The full data were reduced by following the simpler course indicated by Drude. In table 1 are recorded the results

TABLE 1
$\Phi = 60°$

$\lambda(\mu)$	$\Delta_1 - \pi$	Ψ_1	$\Delta_2 - \pi$	Ψ_2	α	γ
.45	29°31′	24°25′	23°11′	35°22′	.03421 − .06887i	.00318 − .04009i
.50	27°54′	23°15′	11°52′	34°14′	.04685 − .06168i	.01853 − .02566i
.55	20°41′	24°26′	4°26′	32°27′	.04773 − .04156i	.02729 − .01378i
.65	21°49′	27° 7′	13°54′	34°12′	.03009 − .04382i	.01451 − .02530i
.70	14°38′	26° 7′	11°24′	31°13′	.04226 − .03180i	.02755 − .02321i

following the simpler calculations. For comparison, in table 2 are the results yielded by the more rigorous calculation.

TABLE 2

λ (μ)	α	γ
.55	.0473 − .0402i	.0268 − .0142i
.70	.0417 − .0306i	.0275 − .0225i

It remains to calculate the optical constants from these values of α and γ. In Drude's[17] dissertation we have

$$\alpha = a_{11} + ia_{12}$$
$$\beta = a_{21} + ia_{22}$$
$$\gamma = a_{31} + ia_{32}$$

With hexagonal crystals, β becomes equal to α, and so has not entered into our present discussion. Further, we have χ and ϵ which are two auxiliary angles defined by

$$\tan \chi = \frac{a_{32}}{a_{31}}, \quad \tan \epsilon = \frac{a_{12}}{a_{11}},$$

then the absorption coefficients are given by

$$k_1 = \tan \chi/2, \qquad k_2 = \tan \epsilon/2,$$

[17] Loc. cit. 1887. There is a change in the use of the letters α, β, and γ in Drude's two papers. Careful reading will obviate any confusion.

and the indices of refraction by

$$n_1^2 = \frac{2\,(\sin X/2)\,(\cos^3 X/2)}{a_{22}},\quad n_2^2 = \frac{2\,(\sin \epsilon/2)\,(\cos^3 \epsilon/2)}{a_{12}},$$

and the reflecting powers by the usual expressions

$$P = \frac{n^2(1+k^2)+1-2n}{n^2(1+k^2)+1+2n},$$

using the subscripts *1* and *2* for the two cases. The results follow in Table 3.

TABLE 3

$\lambda(\mu)$	k_1	n_1	ρ_1	k_2	n_2	ρ_2
.45	.92	3.66	.56	.62	3.06	.39
.50	.51	5.00	.53	.50	3.22	.37
.55	.24	5.56	.50	.37	3.72	.39
.65	.58	5.07	.55	.53	3.84	.44
.70	.36	4.95	.49	.36	4.12	.40
and by the more accurate approximations						
.55	.25	5.57	.51	.37	3.77	.39
.70	.36	4.99	.49	.33	4.18	.42

These data are graphically represented in Figs. 2, 3, and 4.

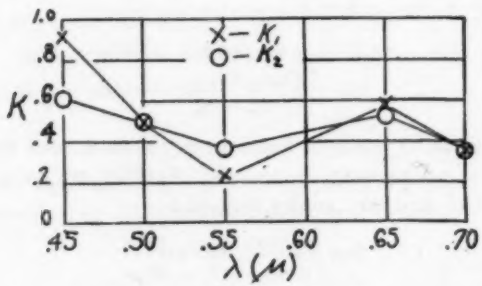

FIG. 2

Variation of the absorption coefficient with the wave-length, in the two principal positions.

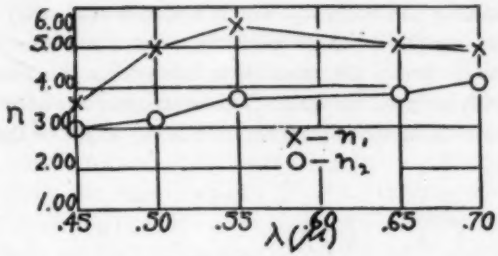

FIG. 3
Variation of the index of refraction with the wave-length in the two principal positions.

III. EXPERIMENTAL DETERMINATION OF THE PRINCIPAL REFLECTING POWERS

Some time ago, following the publication of Skinner's paper, I attempted to check his results by experiment. To measure k and n directly with crystals so small as these is practically out of the question. To use cast selenium as Wood[18] did for the direct determination of n would be begging the whole question. However, the direct measurement of the reflecting powers seemed

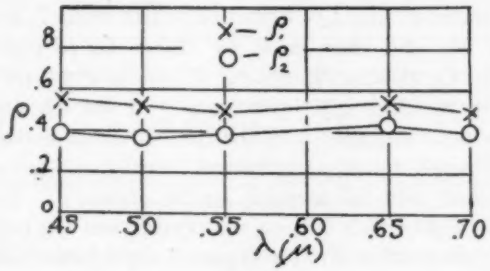

FIG. 4
Variation of the reflecting power with the wave-length in the two principal positions.

feasible and some preliminary results were obtained and published,[19] but they were subject to considerable experimental error, and small reliance was placed in them. Last fall the problem was again attacked, and while it is felt that the results are still far

[18] Wood, Phil. Mag. *3*, p. 612; 1902.
[19] Sieg, Proc. Ia. Acad. Sci. *23*, p. 179; 1916.

from possessing the accuracy which I should like, they are still much more consistent than were the former values. They at least definitely prove the contention concerning the necessity of working with isolated crystals. The arrangement of the apparatus, shown in elevation is sketched in Fig. 5. Light of the desired

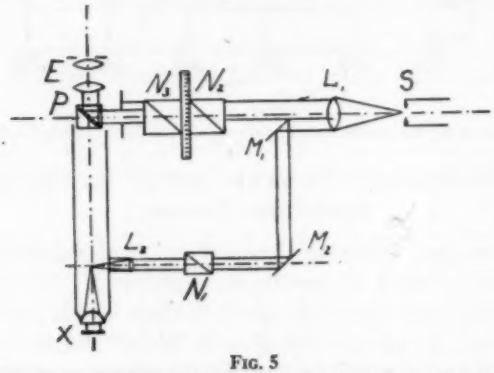

FIG. 5
Arrangement of spectrophotometer for determining the reflecting powers of small surfaces.

wave-length from the slit of the monochromatic illuminator S is made parallel by lens L_1. A portion of this beam is intercepted by mirror M_1, and then made by mirror M_2 to pass through Nicol N_1 to the opaque illuminator of the Saveur type in one of the regular metallographic microscopes of Bausch and Lomb. This plane polarized light with the electric vector horizontal was reflected through the objective system (usually an 8 mm objective was employed), and fell normally on the crystal X. In fact this is a cone of light which falls on the crystal, but the angle of the cone is only about 3°, and so for all practical purposes the incidence is normal. The crystal X is mounted on the stage in a special holder, and can be turned,without loss in centering, or in maintenance of plane, so that its long axis is either parallel or perpendicular to the electric vector of the incident polarized light. The reflected light passes up through the tube and forms a real image of the crystal surface on the interface of the double prism P. This image owes its intensity to the magnitude of the reflecting power of the crystal in the particular position it happens to

occupy. The double prism P consists of two small right angled prisms, one of them silvered with the silver film cut into a grid by removing alternate narrow strips of the silver. That portion of the image passing between the strips is viewed by the eyepiece E. The upper portion of the beam from L_1 passes through the two Nicols, N_2 and N_3, illuminating the silver strips, which are viewed by the same eyepiece E. By adjusting the Nicol N_2 (N_3 set to make the electric vector horizontal) a match in intensity can be made for each wave-length, and for each position of the crystal. With this arrangement, as far as described, only the relative reflecting powers in the two principal positions can be determined. In order to obtain the absolute reflecting powers a piece of glass, backed with silver, is substituted for the crystal at X. The absolute reflecting powers of glass, backed with silver,

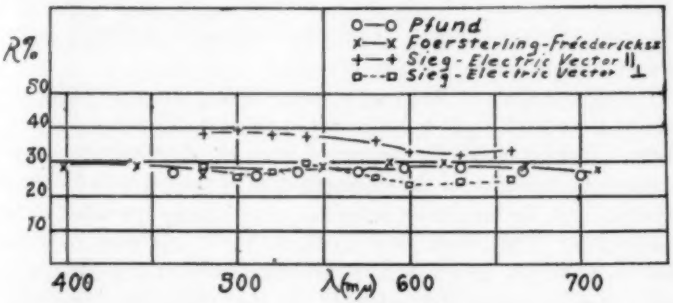

FIG. 6

Experimental determination of the principal reflecting powers of an isolated selenium crystal, compared with previous results on cast selenium plates.

being easily found in physical tables, it becomes a simple matter to translate relative reflecting powers into absolute ones. On account of the loss in light resulting from dividing the beam, and using fairly high magnification, it was not found possible to extend the results very far toward either the red or violet ends of the spectrum. Individual sets of observations were somewhat difficult to repeat with any great accuracy, but by making many different settings, and using several different crystals it is felt that the mean results are correct to 5 to 7 per cent. The extensive original

data will not be presented here. The final mean values are shown in graphical form in Fig. 6. The curves are largely self-explanatory. The data of Foersterling and Freedericksz,[20] and of Pfund[21] are recorded for the sake of comparison with the results of other observers on crystalline selenium in the form of polished plates. The essential point to re-emphasize is that here we have double reflecting powers and that in previous work we have single reflecting powers. That former values are not exactly half way between these double values is of no great moment. An exact mean would imply that the crystals were all lying on their sides, and haphazardly arranged. If any crystals were inclined to the reflecting surface of the plate, then the smaller reflecting power would predominate. In fact recently Grippenberg[22] concludes that in the process of crystallizing selenium plates, most crystals are formed "end on" to the plane surface. This agrees well with the results shown in Fig. 6, as previous values are distinctly below the average position of my results.

It happens that my data agree somewhat better with Skinner's results than with Weld's, but there is no vast difference between them. Weld was chiefly concerned with the development of an accurate instrument for the determination of optical data for small crystals, and his results on selenium were largely intended to be illustrative. They were very accurate, but if one were to make that the chief job, he would have to obtain more data, particularly using various angles of incidence. At that, however, I consider the calculated values from Weld's data as more reliable than the experimental results. I have been pleased enough to find that experiment yielded results of the proper order of magnitude.

UNIVERSITY OF IOWA,
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[20] Ann. der Phys. *43*, p. 1227; 1914.
[21] Pfund, Phys. Zeit. *10*, p. 340; 1909.
[22] Grippenberg, Phys. Zeit. *22*, p. 281; 1921.

SOME OF HUYGENS' CONTRIBUTIONS TO DIOPTRICS, WITH NOTES

BY
JAMES P. C. SOUTHALL

The beginning of the seventeenth century is a notable epoch in the history of optical science. The greater astronomer Tycho Brahe died in 1601 before the invention of the telescope which in the hands of Galileo (1564–1642) and his contemporaries and successors led to so many celestial discoveries. By 1604 Kepler (1571–1630), who is sometimes called the "father of modern optics" had published his "Paralipomena in Vitellionem seu Astronomiae pars optica," in which after many painstaking efforts he had obtained with characteristic perspicacity and ingenuity a "one constant" formula for the law of refraction which had baffled all previous investigators, and which may be written as follows:

$$i-r=C\cdot i\cdot\sec r,$$

where i, r denote the angles of incidence and refraction and where C is a constant factor which expressed in terms of the so-called relative index of refraction (n) of the two media apparently has the following form:

$$C=\frac{n-1}{n},$$

as has been pointed out by Dr. Houstoun in a paper on "The Law of Refraction" published in *Science Progress* (January 1922). By means of this formula, Kepler, using the value $n=1.317$ for the index of refraction of water, computed (by an ingenious process of successive approximations, as Houstoun states) the values of the angle of refraction (r) corresponding to arbitrarily assigned values of the angle of incidence (i); and was able to show that the calculated results were in fairly close agreement with the measurements which Vitellio had made about 1270. When the angles

are so small that we may write sin $i = i$, sin $r = r$, Kepler's formula becomes $i = n.r$, in perfect agreement, therefore, with the exact formula sin $i = n.\sin r$ under the same circumstances. Both Alhazen (who is said to have died in 1038) and Vitellio tried to find by their experiments a law for the ratio of the corresponding angles of incidence and refraction, and they were aware that this ratio was approximately constant for comparatively small values of these angles. A Jesuit writer named Kircher in a book called "Ars magna" published in 1646 gives a table of corresponding values of the angles of incidence and refraction for air-water as found by Christoph Scheiner (1573–1650), who was a contemporary of Kepler and Galileo and like them one of the pioneers in the development of optical science. But although neither Kepler nor Scheiner succeeded in ascertaining the true law of refraction, it is indeed remarkable what keen insight into the nature of optical phenomena each of these men possessed and how accurate their conclusions were in most instances. No one can read Kepler's famous little treatise entitled "Dioptrice" (1611) or Scheiner's even more extraordinary work called "Oculus sive fundamentum opticum" (1619) without being impressed again and again by this fact. One explanation of their achievements may be found perhaps in the circumstance that the optical problems which they encountered were concerned chiefly with the procedure of the so-called paraxial or central rays for which the approximate formula $i = n.r$ is sufficiently accurate. In Prop. LIX of the "Dioptrice," for example, Kepler states that "the surface of a dense medium which will refract parallel rays to a real focus in a less dense medium is approximately hyperbolic." Had he but known the exact law of refraction, he could have omitted the word "approximately"!

Christiaan Huygens (1629–1695) was more fortunate in this regard than his predecessors mentioned above; for by the time he came of age the law of refraction in terms of the sines of the angles of incidence and refraction had been announced by Descartes (1596–1650), although the law itself was probably discovered first by Huygens' own countryman Snellius (1591–1626). At intervals during nearly the whole of an illustrious and fruitful

career in nearly every branch of natural philosophy Huygens was at work on a treatise on "Dioptrica" containing his theorems and original contributions in this domain of optical science. It was begun in 1652 when he was twenty-three years old, and he was continually adding to it, revising it, and sometimes planning to rewrite it entirely in consequence of new discoveries and new points of view; and so it was never actually completed. His famous "Traité de la lumière," which was written as early as 1678 but not published until 1690, does not attempt to go in detail into the theory of mirrors and lenses, and is entirely distinct from the "Dioptrica." This latter work in the imperfect state in which Huygens left it appeared first in the posthumous edition of his writings which was published in Leiden in 1703. In 1888 the Dutch Society of Sciences began the publication of the monumental edition of Huygens' complete works. The thirteenth volume[1] entitled "Dioptrique" was issued in 1916 and comprises, with introduction, notes, etc., more than a thousand pages. Besides containing some hitherto unpublished portions of the "Dioptrica," the valuable explanations and comments of the accomplished editor contribute to make this huge volume a veritable depositary of varied and accurate information particularly in regard to the early development of optics.

Unfortunately, not a few of Huygens' original and sometimes most valuable theorems in optics did not come to light until long after they were obtained. Consequently, he lost the priority of a number of important discoveries. It would be beyond the scope of this paper to enumerate them all, much less to discuss each of them in detail.

In the very first part of the "Dioptrica," Huygens treats at great length the problem of refraction at a spherical surface. It is worth noting with what elegance and skill—far in advance of his contemporaries in this respect—Huygens derives from the recently discovered law of refraction the fundamental laws of optical imagery in the limiting case when the effective rays are nearly normal to the refracting surface. Thus, for example, for a

[1] Œuvres complètes de Christiaan Huygens publiées par la Société Hollandaise des Sciences. Tome treizième. Dioptrique. La Haye; 1916.

pair of conjugate points (M, M′) on the axis of a spherical refracting surface (with its vertex at A and centre at C), Huygens gives the following general rule:[2]

$$MF : MA = MC : MM',$$

where F designates the position on the axis AC of the first focal point (Huygens calls the focal points "puncta concursus vel dispersus"); that is, the distances measured from the axial object-point M to each of the points F, A, C and M′ taken in order form a proportion. Likewise, in the case of a lens of negligible thickness with its optical centre at a point which may be designated here equally by A or C, the rule becomes:

$$MF : MA = MA : MM'.$$

If these propositions had been published about 1653 when they were first obtained by Huygens, he would certainly have had the priority for them. They were communicated in an anagram to the Royal Society in 1669, but at that very time Dr. Isaac Barrow's "Lectiones Opticae" was in the press, in which were to be found essentially the same theorems derived in a different way. But Huygens had the idea of equivalent lenses which Barrow did not. When the "Dioptrica" was first published in 1703, other writers also, notably Molyneux and Halley[3] in England, had given rules which were practically the same as those of Huygens.

Of much interest too is Huygens' way of defining and measuring the magnifying power of an optical instrument, by which he means the ratio of the apparent size of the object as seen through the instrument of its apparent size as presented to the unaided eye. This ratio is found by comparing the dimensions obtained by projecting the object in each instance on a fixed plane at right angles to the line of sight, for example, on a transversal plane in contact with one of the lens-surfaces. One advantage of this mode of reckoning is that it can be employed for any position of the eye, no matter whether the image can be seen distinctly or not by an

[2] *Loc. cit.*, pp. 40, foll.

[3] Molyneux's "Dioptrica nova" (1692), pp. 42, 48, 63 and 68. Also article by Halley on "An Instance of the Excellence of the Modern Algebra, in the Resolution of the Problem of finding the Foci of Optick Glasses universally": *Phil. Transactions*, *17*, pp. 960-969; 1693.

actual eye at the place in question. The general method can be illustrated in a simple way by an optical system composed of a thin lens in combination with the eye of an observer. Let O, O′ designate the positions of a pair of conjugate points on the axis of the lens, and let f denote the focal length of the lens, reckoned positive or negative according as the lens is convergent or divergent, respectively. Suppose that the observer's eye (or, more exactly, a specified point in his eye) is placed at O′. An incident ray proceeding from a point Q in the object and directed towards (or away from) the point O will meet the lens at a point B and be bent there into the eye at O′. Let K designate the point where the straight line QO′ meets the lens, and let A designate the optical centre of the thin lens; then the ratio AB : AK is Huygens' measure of the magnifying power for this particular position of the eye. Finally, if M designates the foot of the perpendicular let fall from Q on the lens -axis, and if $u = \mathrm{AM}$ and $c = \mathrm{AO}'$ denote the distances of M and O′ from the lens, it may easily be seen that for this optical system the following formula holds:

$$\text{Magnifying power} = \frac{\mathrm{AB}}{\mathrm{AK}} = \frac{(c-u)f}{uc+(c-u)f}.$$

This expression is entirely general and true for a convergent lens ($f>0$) or a divergent lens ($f<0$) and for a real or virtual object; provided only the abscissae u, c and f are reckoned positive or negative according as they are measured one way or the other along the axis. Naturally, in any actual case the point O′ where the eye is placed will be always on the far side of the lens. Huygens himself treats each case separately, but, as has been said, the results can all be summed up in the above formula. Incidentally, it may be remarked that the points designated here by O, O′ in nearly all actual cases of a single lens used in conjunction with the eye correspond to the centres of the so-called "entrance-pupil" and "exit-pupil" of the compound optical system. Huygens was perfectly aware of the convenience and importance of these points and of the effect of the pupils in limiting the apertures of the bundles of effective rays; and he constantly makes use of these ideas which are usually regarded as quite modern.

The beautiful theorem which states that the apparent size of an object as seen through a lens-system will not be altered when the positions of the eye and the object are mutually interchanged, and which is given in Robert Smith's "Compleat System of Opticks" (London, 1738), is to be found[4] in the second book of the first part of Huygens' "Tractatus de refractione et telescopiis" (1635); and although it was not actually published until 1703, there can be no question that Huygens is entitled to the priority here.[5] Moreover, he constantly makes use of this general principle in the solution of optical problems. For instance, in the third book of the treatise on refraction and telescopes (pp. 255–257, 261) this theorem is employed to show that the magnifying power of a telescope may be measured by the ratio of the diameter of the object-glass to that of the so-called "eye-ring" which is the image of the object-glass in the ocular. In fact, in order to see this, we have merely to suppose that the eye is placed at the centre of the "eye-ring" while the object is infinitely far away. If now the positions of eye and object are supposed to be reversed, a little reflection will show that according to Huygens' theorem the magnifying power of the instrument must be equal to the ratio above mentioned.

In connection with the problem of refraction at a spherical surface, Huygens repeatedly[6] calls attention to the existence of a remarkable pair of points J, J′ which lie on any straight line passing through the centre C of the surface and which are distinguished by the fact that *all* incident rays which intersect, "really" or "virtually," in J will, after refraction, intersect again, "virtually" or "really," respectively, in the corresponding point J′. The positions of these points may be precisely defined as lying on a straight line drawn through the centre C and at distances from C such that

$$CJ = n \cdot AC = n \cdot r, \quad n \cdot CJ' = AC = r;$$

where A designates the point where the straight line meets the side of the surface on which the light falls, and where n denotes the relative index of refraction from the first medium to the

[4] *Loc. cit.*, pp. 198, foll.

[5] See J. O. S. A. & R. S. I., *6*, p. 293, 1922.

[6] *Loc. cit.*, pp. 48, foll.

second and r, as usual, denotes the radius AC. The points J, J′ are thus seen to lie both always on the same side of the centre and on the opposite sides of it from the point A. If the incident rays intersect "really" in J, the refracted rays will intersect "virtually" in J′, and *vice versa*. According to the above definition, we must have the relation:

$$CJ \cdot CJ' = r^2;$$

which means geometrically that the points J, J′ are the "inverse" points of the sphere and are therefore harmonically separated by the end-points A, B of the diameter AB on which they lie. The points J, J′ on the optical axis are the pair of so-called aplanatic points of the spherical refracting surface; and in fact they are "aplanatic" not merely in the sense that the surface is absolutely free from spherical aberration with respect to them but also in the wider meaning of that term as it was afterwards employed by Abbe and his disciples; because the rays which are refracted at a spherical surface from J to J′ likewise satisfy the so-called "sine condition."

The first portion of Huygens' "Dioptrica" in which particular attention is called to this unique pair of corresponding points in the case of a spherical refracting surface was composed by Huygens in 1653, but Huygens takes pains to say that they had been discovered by him "a long time ago," and recalls a letter which he wrote to van Schooten in 1652 wherein he explained how under certain circumstances one of the ovals of Descartes becomes "a perfect circle."[7]

The first practical application of the properties of the aplanatic points of a spherical refracting surface was made by Amici (about 1840) in the construction of high-power microscope objectives. Perhaps this is why the discovery of these points was formerly attributed to him. But Thomas Young had called attention to them as early as 1802 and most modern writers assign the credit to him. A recent writer[8] states that Young re-discovered them

[7] He makes the same statement again in his "Treatise on Light," the last chapter of which is devoted to a discussion of the Cartesian ovals and the forms of aplanatic optical surfaces and aplanatic lenses. See S. P. Thompson's English translation, London, p. 114; 1912.

[8] M. L. Dunoyer: "Optique ondulatoire et optique géométrique," *Journ. de Phys.*, Ser. VI, *2*, pp. 258-264; 1921.

after Huygens and suggests that "perhaps Descartes himself was aware of them through his ovals."

Perhaps it will not be without interest to show that a sphere is a particular form of the general aplanatic surface. The condition that a refracting surface shall be aplanatic with respect to a given pair of points J, J′, that is, the condition that light proceeding from (or towards) a prescribed point J in one medium shall be accurately refracted to another prescribed point J′ in a contiguous medium, is:

$$n \cdot PJ' - PJ = \text{a constant},$$

where P designates the position of a point on the surface in question and n denotes the relative index of refraction. The form of the required surface may readily be found in certain special cases. For instance, when the constant above is equal to zero, the condition becomes:

$$n \cdot PJ' = PJ.$$

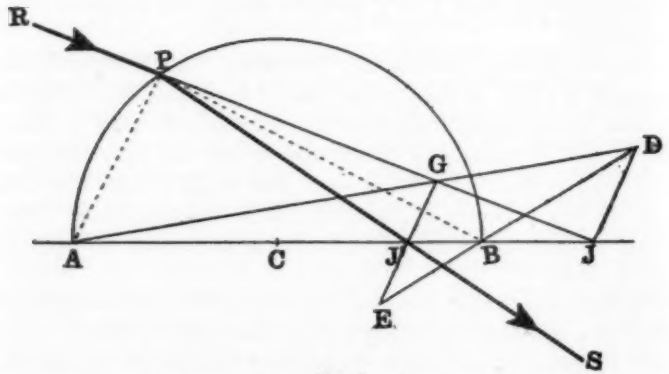

FIG. 1
-Construction of Aplanatic Points J, J′ of a Spherical Refracting Surface: AC=r, CJ=nr, CJ′=r/n.

The locus of the point P which satisfies this condition is a problem of elementary geometry which may be solved as follows:

Through the given points J, J′ (Fig. 1) draw a pair of parallel straight lines JD, J′G. On the first of these lines take a point D, and on the other line find a point G on the same side of the straight line JJ′ as D such that JD : J′G = n. Produce GJ′ on the other

side of JJ′ to a point E such that EJ′ = J′G; and therefore also JD : EJ′ = n. Draw the straight lines DG and DE meeting JJ′ in A and B, respectively. Since the triangles AJD, AJ′G and BJD, BJ′E are two pairs of similar triangles, it follows that

$$\frac{AJ}{AJ'} = n = \frac{BJ}{J'B};$$

and consequently A and B are seen to be two points on the required locus. Suppose P is another such point; then by hypothesis PJ : PJ′ = n; and therefore PJ : PJ′ = AJ : AJ′ = BJ : J′B. Accordingly, PB and PA must be the bisectors of the internal and external angles at P in the triangle JPJ′. Hence, the angle APB is a right angle and the locus of P is the circle described on AB as diameter. Therefore the aplanatic surface in this case is a sphere

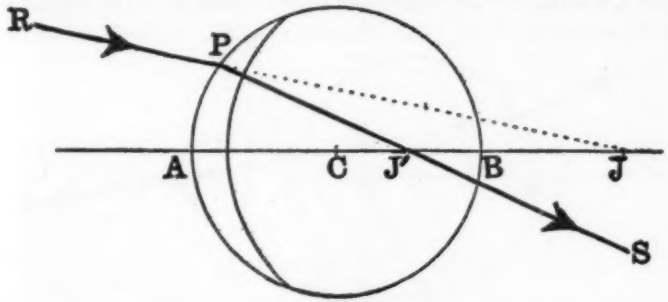

FIG. 2
Aplanatic Glass Lens: AC = r, CJ = nr, CJ′ = r/n (where n denotes index of glass).

It may be remarked that when the point P is so situated on the circumference of the circle that the sides PJ and CP of the triangle CPJ form a right angle, PJ′ will also be perpendicular to CJ at J′. Consequently, the effective portion of the spherical refracting surface above the diameter AB will be the part comprised between the vertex A and this limiting position of the incidence-point P. Thus the maximum apertures of the two homocentric bundles of corresponding rays will be equal to twice the critical angle for the two media and the angle π or 180°.

Huygens was not content merely with discovering the existence of the pair of aplanatic points of a spherical refracting surface; but with characteristic thoroughness he proceeds to describe the

form of a glass lens with spherical surfaces which will be aplanatic with respect to a prescribed pair of points J, J′. The axis of the required lens will be determined, of course, by the straight line JJ′. Having divided the line-segment JJ′ externally at A and internally at B in the same ratio so that AJ : AJ′ = BJ : J′B = n, and having described a circle on AB as diameter (just as above), we may describe a second circle around J′ as centre with a radius somewhat less than J′A. This circle will determine by its intersection with the first circle the form of a meridian section of a convex meniscus lens (Fig. 2) which will be aplanatic with respect to J and J′. Or merely interchanging the letters J and J′ without changing the first circle, we may describe a second circle around J as centre with a radius somewhat greater than JA, and so determine the section of a concave meniscus lens which will be aplanatic with respect to J and J′, as shown in Fig. 3. In both forms the

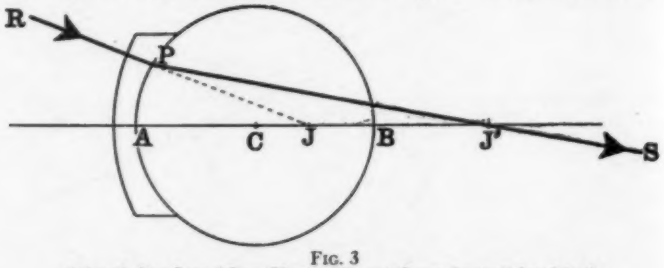

FIG. 3
Aplanatic Glass Lens: AC = r, CJ = r/n, CJ′ = nr (where n denotes index of glass).

two aplanatic points will lie on the same side of the lens, so that we can construct: (1) A convex meniscus lens which will render a converging (or diverging) bundle of incident rays more convergent (or less divergent); and (2) A concave meniscus lens which will render a converging (or diverging) bundle of incident rays less convergent (or more divergent); in each case supposing that the rays fall first on the convex (or concave) side of the lens.

One other point remains to be mentioned in connection with this subject. Both Huygens in his "Dioptrica" and Isaac Barrow in his "Lectiones opticæ" (1669)[9] are at pains to give a special

[9] See Whewell's edition of Barrow's "Mathematical Works" (Cambridge, 1860), "Lectiones opticæ," Lect. XI, II, p. 96.

geometrical construction for the important case when a ray is refracted at a spherical surface. Barrow's method is much more complicated than the simple and elegant construction which is to be found in Thomas Young's "Lectures on Natural Philosophy" (London, 1807) and which was obtained independently long afterwards by Weierstrass (1858) and still later by Lippich (1877). The accompanying diagram (Fig. 4) shows Huygens' own drawing

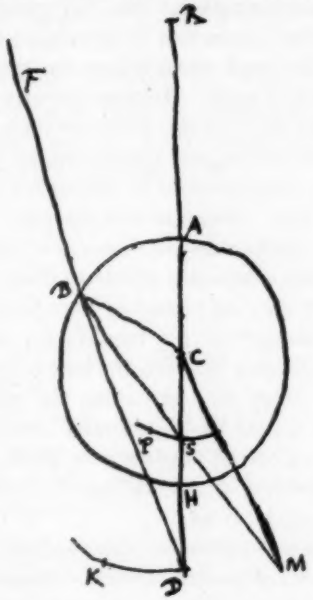

FIG. 4

Huygens' own diagram for showing construction of ray refracted at a spherical surface, by aid of aplanatic points D,S; reproduced from Fig. 27, Vol. XIII of Huygens' *Œuvres complètes*, p. 63.

and lettering in which the pair of aplanatic points on the axis RD of the spherical refracting surface are designated by D and S. Huygens says that if the straight line FB represents an incident ray directed towards the point D and meeting the spherical refracting surface in B, then BS will be the corresponding refracted ray; and "it is manifest that if two spherical surfaces are supposed to be described around the centre C with radii CD and CS, and

if we take two points K, P on these surfaces lying on the same radius CK, all rays aimed towards the point K will be refracted at the surface ABH of the transparent body so that they will converge exactly towards the point P." This is certainly entirely equivalent to Young's construction, although it should not be necessary to draw the straight line CM parallel to the incident ray FB, in order to prove it, as Huygens does.

The early telescope-makers, like Huygens, encountered the problem of spherical aberration in the object-glass of the instrument, and they bestowed much labour on trying to improve this troublesome source of error. Huygens devotes a large part of the "Dioptrica" to the study of this intricate subject and some of the formulæ which he derives are equivalent to those found in the modern treatises. But it would be impossible to go in detail into all these matters here. Huygens had also very clear and accurate ideas about the mechanism of vision and physiological optics which it would take a separate article to discuss properly.

In conclusion it may be remarked here that a very good idea of the spherical aberration of a lens-system can be obtained by calculating the difference between the lengths of the optical paths along a so-called "edge-ray" and along the optical axis. As this method does not appear to be generally known, perhaps a brief outline of it will not be out of place here, particularly as it is connected with the subject of aplanatism which has been discussed above in certain special cases.

Consider a centered system of m spherical refracting surfaces; let the centre and vertex of the kth surface be designated by C_k and A_k, respectively, where k may denote any integer from 1 to m, inclusive. Let n_k denote the absolute index of refraction of the medium comprised between the $(k-1)$th and kth surfaces. The axial thickness of the medium of index n_{k+1} will be denoted by $d_k = A_kA_{k+1}$; and the radius of the kth surface by $r_k = A_kC_k$. A ray of light crossing the axis in the first medium at a point L_1 at an angle θ_1 meets the first surface at a point B_1 whose distance from the axis will be denoted by h_1; this ray is refracted from one surface to the next and crosses the axis before refraction at the kth surface at a point L_k making with the axis an angle $A_kL_kB_k = \theta_k$,

where B_k designates the point where the ray meets the kth surface. The distance of the point B_k from the axis is denoted by

$$h_k = r_k \cdot \sin(\alpha_k - \theta_k),$$

where α_k denotes the angle of incidence at this surface; the angles α_k and θ_k being both acute angles and reckoned positive or negative according as the sense of rotation is counter-clockwise or the reverse. Finally, the ray emerges at the last surface and crosses the axis at the point L_{m+1}. The length of the optical path from L_1 to L_{m+1} along the axis is:

$$n_1 \cdot L_1A_1 + n_2 \cdot A_1A_2 + n_3 \cdot A_2A_3 + \ldots + n_{m+1} \cdot A_mL_{m+1};$$

whereas the length of the optical path between the same two points along the "edge-ray" will be given by an identical expression except that the letter A is replaced by B.

Evidently,

$$L_1B_1 = \frac{h_1}{\sin\theta_1}, \qquad B_mL_{m+1} = -\frac{h_m}{\sin\theta_{m+1}},$$

and

$$B_kB_{k+1} = \frac{h_{k+1} - h_k}{\sin\theta_{k+1}}.$$

Consequently, if we put

$$A_1L_1 = v_1, \qquad A_mL_{m+1} = v'_m,$$

the difference in length between the optical paths along the "edge-ray" and along the axial ray, expressed in terms of the above symbols, will be:

$$n_1\left(\frac{h_1}{\sin\theta_1} + v_1\right) - n_{m+1}\left(\frac{h_m}{\sin\theta_{m+1}} + v_m'\right) + \sum_{k=1}^{k=m} n_k\left(\frac{h_k - h_{k-1}}{\sin\theta_k} - d_{k-1}\right).$$

Accordingly, first of all, the path of the "edge-ray" must be traced through the system by trigonometric computation, whereby the values of the angles α and θ will have been ascertained for each surface, so that these values, together with the corresponding values of h, can be substituted in the expression above. If one of the surfaces is plane, then for that surface $\alpha = \theta$ and h is to be calculated by the formula $h = -v \cdot \tan\theta$, $(r = \infty)$, where v denotes the

distance from the plane surface of the point where the ray crosses the axis before refraction at this surface. If the expression above vanishes for the values found in this way, the system will be aplanatic, that is, free from spherical aberration, with respect to the points L_1 and L_{m+1}. In general, however, this will not be the case, that is, the system will be "spherically under-corrected" or "spherically over-corrected" according as the expression above is found to be positive or negative, respectively. It may be possible to make some more or less slight alteration in the optical system, for example, to change the curvature of one of the surfaces, that is, "bend the surface," in such fashion that the difference between the two optical routes will tend to be diminished; and thus gradually we may contrive to vary the system until the spherical aberration has been practically abolished. This is, indeed, a very serviceable method for this purpose. If the variations are introduced in the last member or in one of the latter members, the necessary trigonometrical computations will be reduced, because for all the preceding elements the calculation remains the same.

The expression given above for the difference in length of the optical paths along the "edge-ray" and along the axial ray may also be put in another form as follows:

$$-\Sigma\frac{n\cdot p\cdot E}{\Pi},$$

where the magnitudes denoted by n, p, E and Π must be known for each surface. Here p denotes the length of the perpendicular let fall from the centre of the surface on the incident ray and n denotes the absolute index of refraction of the medium which is traversed by the incident ray. The magnitudes E and Π are certain functions[10] of the angles of incidence and refraction (α, α') and the slope-angles (θ, θ') of the ray before and after refraction at each surface, which are connected by the invariant-relation:

$$\alpha'-\theta'=\alpha-\theta.$$

These functions are defined as follows:

[10] The function denoted here by E is precisely the same as was employed in a previous paper by the author: *Journ. Opt. Soc. Amer.*, *4*, pp. 294-299; 1920.

$$E = (\sin\alpha - \sin\theta) - (\sin\alpha' - \sin\theta')$$

$$= -4 \sin\frac{\alpha-\theta}{2} \cdot \sin\frac{\theta-\theta'}{2} \cdot \sin\frac{\alpha+\theta'}{2};$$

$$\Pi = 4 \cos\frac{\alpha}{2} \cdot \cos\frac{\alpha'}{2} \cdot \cos\frac{\theta}{2} \cdot \cos\frac{\theta'}{2}.$$

When one of the surfaces is plane, then $\alpha = \theta$, $\alpha' = \theta'$ and consequently both E and Π vanish. In this case it will be found that

$$\frac{n \cdot \rho \cdot E}{\Pi} = \frac{n \cdot h \cdot \sin\theta \cdot \sin\frac{\theta-\theta'}{2} \sin\frac{\theta+\theta'}{2}}{2 \cos^2\frac{\theta}{2} \cos^2\frac{\theta'}{2}}, \quad (r = \infty).$$

DEPARTMENT OF PHYSICS,
COLUMBIA UNIVERSITY, NEW YORK, N. Y.,
APRIL 15, 1922

ACCURACY IN COLOR MATCHING OF INCANDESCENT LIGHT SOURCES

BY
W. E. FORSYTHE

The relative distribution of energy in the visible spectrum of various radiating bodies for different temperatures has long been a matter of interest. There are two methods for obtaining directly such relative distributions in the visible spectrum, the one is by using some form of a spectrophotometer where the comparisons are made by eye observations, the other by using one of the methods where the energy is compared by means of some sort of an energy measuring device.

Owing to the decreasing sensibility of the eye, observations with the spectrophotometer are very difficult at the extreme red or blue end of the spectrum. It is also very difficult to make accurate measurements with a bolometer or thermopile in the extreme blue end of the spectrum.

There is a third method of obtaining such relative distributions that has some advantages over the two mentioned above. This method consists in matching in color the light from the source studied with that from a black body or other standard source. Such comparisons can be very accurately made with the ordinary Lummer-Brodhun contrast photometer. To do this the source studied is mounted on one side of the photometer and a comparison lamp on the other side. Suppose that at the start the comparison lamp is too low; then when there is a brightness match the trapezoid that is illuminated by the comparison lamp will appear reddish as compared with the other, which appears bluish. If now the voltage applied to the comparison lamp is raised a small amount and at the same time the photometer moved so that there is an intensity match, as observed in the photometer, the comparison trapezoid will, if the change has not been too great, appear less reddish than before. By repeating this process the comparison

lamp can be very accurately matched in color with the source being studied. The source being studied is now to be removed and replaced with the standard lamp and the process repeated just as before excepting that in this case the voltage applied to the standard lamp is to be varied and the comparison kept constant at the voltage above obtained. Thus the standard source can be brought to a color match with the comparison source. In this way the source studied and the standard lamp are color matched by the substitution method. It has been found experimentally that a black body and most radiating solids can thus be color matched. Also most of the flames that have been used as light sources can be color matched with the black body. However, the Welsbach gas lamp and the sources that do not have a continuous spectrum cannot be so compared. For any source that can be color matched with the standard, the distribution of energy in the visible spectrum can be obtained much more accurately and with much less work than by either of the other methods, as will be shown below.

Color matching by different observers. In order to see how different observers would agree in their setting, two lamps were color matched by the substitution method by a number of observers. All the observers except one made three sets of five readings each on the voltage of the two lamps for color match with a third lamp of the same type held at a constant voltage.

In Table 1 are given the results of this test. The first three observers as listed in the table had had considerable experience with this kind of work. The fourth observer had had some previous experience while the last two had had no previous experience. The color temperature of the lamps used in this and the following test was about 2400° K. The illumination on the photometer screen for this test was 5.1 foot candles. The averages of the voltages obtained in each set by each observer for lamp No. 4 to color match lamp No. 5 at 105 volts are given. There is also given the mean variation in volts for the different readings for the different observers. An examination of the table will show that the maximum range in the three sets for any of the experienced observers is less than 0.4 volt. The range in the

TABLE 1

Values obtained for color matching lamp No. 5 against lamp No. 4 by a number of observers

Observer	Volts for color match mean of three readings	Mean of three sets	Mean variation in volts
F.E.C. 1	103.39		
2	.60		
3	.71	103.57	0.19
A.H.T. 1	103.72		
2	.71		
3	.66	103.69	0.25
W.E.F. 1	103.80		
2	.45		
3	.79	103.68	0.13
R. S. 1	103.78		
2	.47	103.63	0.31
R.D. 1	104.43		
2	103.30		
3	.51	103.75	0.20
I.K. 1	103.15		
2	103.49		
3	102.81	103.15	0.28

averages of the three sets for these same observers is only about one tenth volt. The maximum range in the averages for all the observers is only about five tenths volt. A change of one volt at this point corresponds to a change in color temperature of about 8° K. It can thus be seen that the maximum range for inexperienced observers corresponds to about 4° K. A range of 4° K at this temperature corresponds to an error of about 0.6% in relative energy between the red ($\lambda = .665\ \mu$) and the blue ($\lambda = .467\ \mu$). This is much better than the accuracy generally claimed in spectral distribution work.

The photometer used in making the color matches is of the Lummer-Brodhun contrast type having a contrast of about eight per cent. The reason for using this type is because it has been

found to be much easier to make color matches with a contrast photometer. However, such matches can be made with an ordinary equality of brightness photometer. To see if any differences would be obtained several sets were made with a photometer having a contrast of about three and one half per cent. Although it seemed much harder to make color matches with this photometer, the final results were in very good agreement with those obtained with the other photometer.

Color matching at different intensities of illumination on the photometer screen. A test was also made to find out what accuracy could be expected in such work for different intensities of illumination on the photometer screen. In this test, different observers made quite a number of color matches for different illuminations on the photometer screen using in every case two tungsten lamps so that there would be no question as to the exactness of color match. The range of illumination on the photometer screen was from 1.8 ft. candles to 45 ft. candles.

Since it would require too much time and be too great a strain on the eyes for an observer to make readings at each of the intensities of illuminations at one time the following method was adopted. A set of readings was made each time at an illumination of 2.8 ft. candles taken as the standard and at one other illumination. In this way, the readings for each of the illuminations can be compared with the standard illumination. Two complete sets of readings were made at each illumination by each observer. In the first set, five readings were made at the standard illumination and then five at one of the other illuminations. In the second set of readings, three sets of three readings each were made at the standard illumination and alternating with these two sets were made at one of the other illuminations. The mean variations in volts in the readings on lamp No. 5 for color match with a standard lamp operated at a constant voltage for the different illuminations are given in Table 2. The variations for the standard illumination is given along with the variations for the other illuminations taken in the same set. The observers in the test with one change were the same as those used in the first test. The additional observer had had no previous experience.

If there is any difference in the accuracy depending upon the illuminations, it is not very definitely shown by the data. There are slight indications that observer No. 1 sets better at low illumination and observer No. 3 better at high illumination. However, the difference is but little more than their error in setting.

The average variations of the entire group is given for the different illuminations and they show no very definite advantage for either the high or the low illumination. As before, one volt change corresponds to a change of about 8°K, color temperature.

TABLE 2

Mean variation in readings in volts for color matching at different intensities by a number of observers

Set.....	I		II		III		IV		V	
Illumination in ft. candles	2.8	45.1	2.8	25.8	2.8	11.3	2.8	6.3	2.8	1.8
Observers			Mean variations in volts in readings							
F.E.C.....	.16	.22	.17	.20	.10	.21	.22	.09	.18	.13
A.H.T.....	.22	.21	.24	.19	.15	.24	.28	.12	.17	.15
W.E.F.....	.21	.15	.20	.13	.21	.16	.18	.22	.24	.14
R.H.S.....	.22	.11	.32	.23	.42	.21	.22	.13	.15	.21
L.B.....	.22	.10	.26	.15	.29	.18	.25	.27	.25	.23
R. D.....	.20	.12	.16	.24	.23	.26	.17	.30	.10	.20
Average mean variation at each illumination....		.15		.19		.21		.19	.22	.18

From the above results it can be seen that very good agreement can be obtained in making color matches by different observers. There does not seem to be any great advantage in having a high illumination on the photometer screen. Ordinarily in this laboratory in making color matches an illumination of from four to eight foot candles on the photometer screen is used. As was mentioned above the Lummer-Brodhun contrast photometer having a contrast of about eight per cent is used because it seems easier to make color matches with that contrast than with a contrast of about three and one half per cent. It is our intention to test this matter out using different degrees of contrast both above and below this amount.

The color temperature scale above 2650°K used in this laboratory depends on readings made with an optical pyrometer having first a red and then a blue screen in the eyepiece on a black body. The black body was the one designed by Dr. Worthing[1] of this laboratory and consisted of a small tungsten tube with small radial holes for the purpose of observation.

The temperature of the black body was determined from the reading when the red glass was used as a screen and the ratio of the red to the blue brightness gave the color temperature scale. To explain this, suppose that at a temperature of 2800°K the readings on the black body gave a ratio of red to blue brightness, on an arbitrary scale, of 0.6. Now if at some unknown temperature tungsten was found to give the same red to blue brightness it would be said to have a color temperature of 2800°K. By taking a number of such readings both on the black body and on tungsten a color temperature scale for tungsten was determined. This method of determining color temperature together with the accuracy that can be obtained will be more fully discussed in a paper now in the process of preparation.

At the Bureau of Standards the color temperature scale in this region depends upon radiometric measurements made by Dr. Coblentz in the visible spectrum of a particular 500 watt gas-filled tungsten lamp.[2] Mr. Priest, by using the rotary dispersion of quartz, has extended the scale above the temperature corresponding to the energy distribution found by Dr. Coblentz.

Comparison of color scale of Nela Research Laboratories with that of the Bureau of Standards. Two tungsten lamps were color matched against the color standards of this laboratory and then sent to the Bureau of Standards, where they were color matched against their color standards by Mr. Priest. One of the lamps was then returned to this laboratory where it was again color matched. The Bureau of Standards also sent a high efficiency lamp to this laboratory for a like check. The results are given in Table 3.

[1] A. G. Worthing, Phy. Rev. N. S. *10*, 377; 1917.
[2] J. O. S. A., & R. S. I., *6*, pp. 30–34; Jan., 1922.

TABLE 3
Intercomparison of Color Scales

LAMP	COLOR TEMPERATURE DEGREES KELVIN			
	Nela	Bureau of Standards	Nela	Bureau of Standards
900 watt movie	3091	3085	3083	
500 watt gas-filled			2848	2848

The good agreement obtained in the two laboratories (although the actual figures obtained were probably accidental) by two different methods in two different laboratories shows the agreement possible in such work.

SUMMARY

Two tests have been made of accuracy that can be obtained in color matching. The first test was for different observers. It was found that experienced observers agreed in their values for color match to about 3°K for a color temperature of about 2400°K. The second test was for color matching for different illuminations on the photometer screen. No very great difference was obtained in the accuracy for a range in illumination from 1.8 foot candles to 45 foot candles.

The color temperature scale for a temperature of about 2900°K of this Laboratory was compared with that of the Bureau of Standards and a very good agreement found.

NELA RESEARCH LABORATORIES,
CLEVELAND, OHIO,
MARCH, 1922

INSTRUMENT SECTION

COOPERATION BETWEEN THE MAKERS AND THE USERS OF APPARATUS IN AMERICA[1]

BY
F. K. RICHTMYER

As I was thinking over some remarks which might be appropriate to this occasion, I chanced to recall the address given by Professor Anthony Zeleny upon his retirement as vice-president of Section B of the American Association for the Advancement of Science at the Columbus meeting in 1915. Dr. Zeleny spoke on "The Dependence of Progress in Science on the Development of Instruments." Although the address was written for presentation to a group of Physicists, its reading again at this occasion not only would be very appropriate, but would supply about all that need be said.

Two paragraphs from this address may well serve as the keynote for this evening's discussion:

"Real progress in science ultimately rests upon the establishment of facts. We are sure to stray from reality unless (our reasoning faculties) are continually guided by observation and experiment. Galileo, with his experimental methods contributed more to science than did all the generations preceding him.

"Observations made with our unaided senses limit us to the most superficial aspects of natural phenomena but when we bring scientific instruments to our aid—not only are we enabled to observe more accurately and more systematically all that our senses ordinarily perceive, but we become endowed with new senses that open up fields of knowledge of which otherwise we could not even have dreamed."

[1] Presented on the occasion of the joint dinner of the Association of Scientific Apparatus Makers of America and the American Physical Society in Washington, April 21, 1922. Published by request.

And then after calling attention to the debt which science owes to the chemical balance, the Rowland grating, precision temperature measurement, etc., he continues, "I wonder whether we appreciate what we owe to the great accessibility of manufactured materials. What a luxury we have in insulated wire! How could we do without glass tubing!" Research depends not only on good apparatus; but on the availability of high grade manufactured materials. The rapid progress of science today, as compared with fifty or one hundred years ago, is due in no small part to these conveniences. Faraday to energize his electromagnet, had to build up his battery fresh every morning. Today we close a switch, watch an ammeter, forgetting the source of our electrical energy.

But I take it that no one questions Dr. Zeleny's thesis that science owes much to scientific apparatus and materials; and if to these, then to the manufacturers of apparatus and materials. And this further statement likewise seems to me to need no proof: that one of the most important factors in promoting a healthy growth of Science in America is that these two groups, the makers of apparatus and the users of apparatus, should join hands in building up an industry for the manufacture of whatever the American scientist or teacher may need to supply his eve y demand.

One of the arguments frequently advanced for increasing our facilities for Apparatus manufacture is that in the event of war we should thereby be independent of foreign manufacturers and should be able to furnish our scientists with adequate facilities. One cannot deny the force of this argument, for it has been said that the next war will be a scientists' war and we shall need not only scientists of the highest calibre, but unlimited facilities for them to carry on their work. Three years ago many of us would have been inclined to scoff at this argument. The "Great War" was the last war that the human race would ever see.

But now we are less optimistic over the final abolition of war than we were on January 1st, 1919. We have seen the humiliating spectacle of party politics, of personal ambition, of national

[2] Science, *43*, p. 185; 1916.

jealousy, both here and abroad, influencing men's votes and their better judgment on questions of humanity and world welfare. And we realize that, in spite of the awful experiences of four years, the human race is just what it was in 1914. Truly, we are settling down to "normalcy."

But I believe that arguments of this kind can be pushed too far. There are those of us who feel that one of our sister sciences (I am speaking as a Physicist) has rather over-emphasized the "war cry" reason for developing certain branches of that science. Surely no one will dispute the assertion that the fundamental object of science is not to prepare nations to fight each other, but instead to assist in advancing that rather indefinable thing which we call "civilization." We cannot expect American science to experience a healthy, normal growth unless we keep always foremost in our minds this fundamental object. If we must think of preparation for war, let us push this thought as far in the background as possible. For, just as the experience of a century has taught us that that scientist makes greatest progress who takes up his problems with never a thought as to the application of the results of his investigations to the every-day affairs of life, so American science, from apparatus manufacture to the research laboratory, must base its development on far higher motives than preparing for war. Perhaps the old Pilgrim exhortation "Trust in God, but keep your powder dry" is good modern philosophy.

I do not believe that we have to seek far to find weighty reasons for manufacturing in America all instruments needed by American scientists. I am going to pass over the argument that we must buy in America because our money is earned here, since, whether good economic theory or not, the argument is probably not taken seriously by the average investigator, even though he be intensely patriotic. Nor is the argument that American markets are much more accessible than foreign markets of importance, since it is no more trouble to write and mail an order to Europe than to New York City. And a box received from Europe is as easily unpacked as one from New York.

A far more important line of thought is that which is based upon the conviction, which I at any rate entertain, that a great day

will have been reached in American science when we achieve scientific independence in so far as the manufacture of our tools is concerned. Groups of people are strangely like individuals. The boy who builds his own wireless set gets infinitely more of pleasure and profit than if he were supplied with a ready made set. With the latter he may be able to get more radio. But if the object in providing him with a set is to develop his ingenuity and his general well being ten or twenty years hence he will be encouraged to build his own set even though it take more of his time and much more money.

As with the individual so with the group. With our limited financial resources, we may be able just now to buy more apparatus in the cheaper foreign markets than in America and therewith to do more research. But if we neglect or fail to encourage our own apparatus industry, what will be the effect on American science ten or twenty years hence? Perhaps foreign apparatus always will be cheaper. Perhaps we may always have a foreign supply available. But we must remember that apparatus alone does not make an investigator. Every European visitor to our laboratories marvels at the extent of our equipment. Yet it is universally conceded that, taken as a whole, American research lacks that real fundamental character found in so many centers in Europe. Why this is so I make no attempt to analyze. There are many contributing factors. But certainly the pride and satisfaction which would result from the stamp "Made in America" on all of our apparatus would produce a feeling of confidence which would materially assist our scientific growth. Or to put it in the negative, if one really stops to think of it, one must experience a slight feeling of chagrin when one's investigations, however fundamental, are made possible only by the availability of foreign made apparatus.

I do not wish to be understood as appealing to selfish patriotism. For scientists are perhaps more international than any other group. But nations, like individuals, must retain their own individuality. We can best contribute our quota to world science by contributing it as American scientists. For if, thereby, we can build up a national scientific spirit, we may be able to bring

about a larger appreciation on the part of our public of the value of and the necessity for scientific research. How many, who now pay one hundred dollars for a wireless set and from the morris chair at their own fireside listen to concerts, speeches, weather, market and crop reports, ever give a thought to the decades of research by thousands of men whose combined efforts have produced this modern marvel. The public has become so accustomed to these startling developments that it regards "inventions" as springing full grown from the brain of some scientific genius who locks himself up in his room, conjures with his scientific tools and —behold the result!

I believe, then, that American science will have taken a long step forward when the makers of apparatus, encouraged and supported by the users, set as their goal the manufacture of everything needed in this country, for research and teaching. But if this much desired condition is to be brought about, close and sympathetic cooperation between these two groups, the makers and the users, is absolutely essential. It is obvious that we cannot expect makers of apparatus to take much of an interest, except a financial one, in supplying the demands of the users, if the latter are indifferent or even hostile. On the contrary the users cannot be expected to support the makers if the former feel that financial gain is the sole object of the latter. As a matter of fact, all that is needed is that these two groups should come to know each other better, to appreciate each other's problems, and to view each other's work with sympathetic understanding. Such mutual misunderstandings, if any, as now exist are, I am sure, entirely the result of ignorance on the part of each group, of conditions which the other must face.

For example, to be perfectly frank, the makers of apparatus, quite naturally, are not at all pleased when they see users buying in foreign markets apparatus and supplies which might be purchased here in America. But it is human nature to buy where one can buy cheapest. And I am sure that if the makers of apparatus could see, in detail, the problem which our universities and colleges face, of making very limited financial resources cover the ever growing demand for adequate facilities for instruction and

research, they would realize the pressure thus brought to bear to buy where the market is cheapest.

On the contrary the user of apparatus very frequently underestimates cost of manufacture, particularly if special work is involved. This is perfectly natural since in universities and colleges where mechanicians are at work on special jobs frequently the only costs *apparently* involved are those of materials. No overhead nor interest on investment need be charged; and, in many cases, even the salary of the mechanician is paid out of general university funds, so that no accounting of it need be made on the department budget. To cite a specific instance, a government Department—it was not the Bureau of Standards—recently asked a certain company to build a special piece of apparatus, asking that an estimate be submitted in advance. The quotation submitted was actually one-third of what the company estimated the probable cost to be, charging neither overhead nor profit. The bid was accepted, but with the statement that the price asked was exorbitant! We must not forget that makers of apparatus, in America at least, have neither endowment nor Government subsidy. But that on the contrary they must make a reasonable profit, otherwise their business cannot continue.

It may be possible artificially to stimulate the American apparatus industry by abolishing the privilege of duty-free importation. But I believe that this is not a matter of much importance so far as the present discussion is concerned. No tariff can possibly make the user of apparatus understand the problems which the manufacturer must face. Nor will it give to the maker of apparatus anything more then a passing interest in the user. Far better would it be ultimately to bring about such a condition of mutual understanding and support as would make a tariff *unnecessary*. Whether this can be done depends entirely on future cooperation, the beginnings of which have been made this evening. Cannot some definite steps be taken to get together, and to keep working together, the three groups of people interested in science in America: the manufacturers of apparatus, the scientists who use the apparatus, and, most important, the public which, in the long run, reaps the benefits of scientific work?

ITHACA, N. Y.
APRIL 21, 1922

A FIELD TELEMETER FOR APPROXIMATE SURVEYING[1]

BY
I. C. GARDNER

In the construction of a map on a large open scale there are generally numerous details in the neighborhood of each station which must be located by some approximate method of surveying. To estimate the distances to the points is not sufficiently accurate and to occupy each point by a man with a stadia rod may in some cases be impracticable and in any case involves a large expenditure of time. The problem presented is an ideal one for the self-contained base range finder as developed for military fire control but such an instrument is expensive and furthermore involves the labor of an additional man in carrying it from station to station.

To meet these demands the very light portable device shown in the illustrations was designed. Each instrument consists essentially of a small telescope mounted on a base and with a slow motion screw affording a limited motion in azimuth. When the two instruments are clamped, one at each end of a stadia rod, the whole constitutes a simple self-contained range finder with the stadia rod as a base. Although the angular measurements are necessarily approximate with such a simple piece of apparatus yet the long base enables the requisite accuracy to be obtained for the short distances at which it is intended to be used.

The method of using the instrument is illustrated in Fig. 1. The length AB represents the stadia rod which serves as the base of the triangle. The two telescopes are at the two vertices C and D. After clamping the telescopes on the rod their optical axes are brought into parallelism by means of the slow motion devices on each instrument. The entire assembly comprising the stadia

[1] Published by permission of Director, Bureau of Standards. This instrument was designed and constructed by the Bureau of Standards, at the request of the Topographic Branch, U. S. Geological Survey, Department of the Interior.

rod and the two instruments is then rotated in azimuth as a unit until the telescope C bearing the fixed crosswire is brought upon the target. By means of the micrometer the angle m is measured and from this the distance to the target can be determined by reference to a table computed in advance giving range in terms of the reading of the micrometer screw.

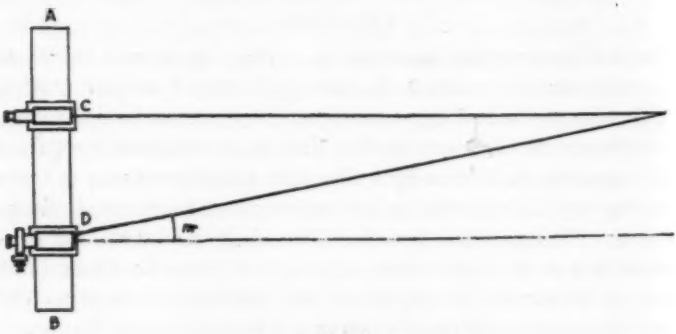

FIG. 1
Fundamental range finding triangle

Some special means is necessary by which the telescopes may be brought into parallelism and for this a device commonly used in the self-contained range finder is adopted. This is shown in Fig. 2. The two penta prisms E and F are placed in front of the telescopes and are of such thickness that they transmit light to the lower half of each objective. On the outer face of each penta prism there is cemented a positive lens as shown at G and H. The focal lengths of these lenses are equal to the distance between the two instruments when clamped on the stadia rod, i.e., 11 feet for the pair of instruments which have been constructed; and each lens bears a vertical mark. If the eye of the observer is placed in the upper half of the exit pupil of either telescope the mark carried on the lens of the other instrument will be in focus. When each instrument is directed so that its cross-wires coincide with the mark on the other instrument the two lines of sight will be parallel if the sum of the deviations of the two penta prisms is 180 degrees. If the penta prisms are not so matched the departure from paral-

lelism can be determined once for all and applied in the calibration table. It should be noted that this method of adjustment is independent of any displacement of the penta prism since any such displacement affects the alignment of both instruments equally. This arises from the fact that the black mark serving as target is carried by the lens. Consequently a shift of the lens moves the target for one instrument and moves the lens through which the other instrument views the target. These two effects compensate each other exactly.

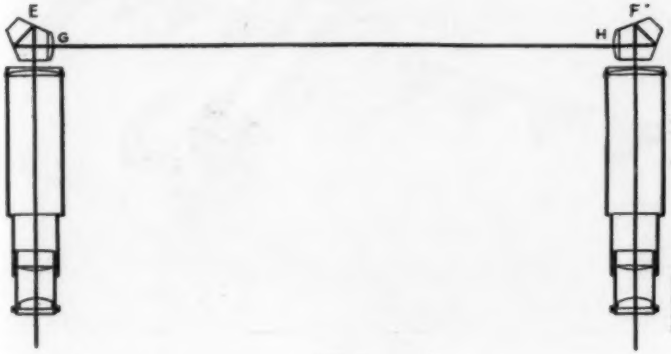

FIG. 2
Method of adjustment

In use the complete assembly will generally be supported at each end on tripods which will ordinarily be available and the observations will be made simultaneously by observers at each end of the stadia rod. The adjustment for parallelism is readily made before each observation. The penta prisms remain in front of the objectives when the observations are made upon a remote target, the upper half of each objective being used in the latter case. It was found convenient to place two washers with spherical surfaces between the base of each instrument and the stadia rod in such a manner that they form a simple ball joint with a restricted range of motion. By means of the ball joint any trouble due to sagging or twisting of the stadia rod is eliminated. It is necessary that the setting for parallelism and the setting on the target be

made at the intersections of the cross wires and the use of the ball joint greatly faciliates this adjustment.

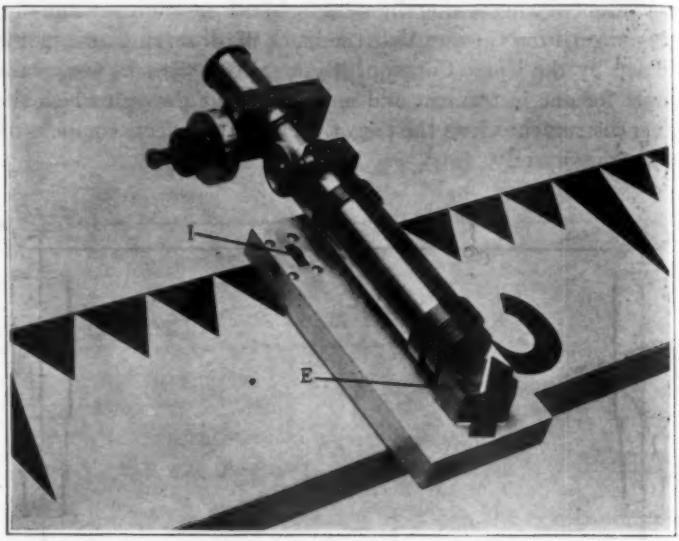

FIG. 3
Instrument mounted on stadia rod

With careful work under favorable conditions, it has been found that the error of angular measurement may be kept under thirty seconds. The corresponding range errors for different ranges will be as follows:

Range Ft.	Error Ft.
1000	13
2000	53
3000	119
4000	211

The telescopes which were used in the sample instruments have objectives 20 mm in diameter and magnify 5.5 times. Appropriate plates are mounted on the stadia rod for the attachment of

the instrument and a base length of 11 feet is provided. Fig. 3 shows one of the instruments on the end of the stadia rod and Fig. 4 shows the pair of telescopes. The penta prisms are at E

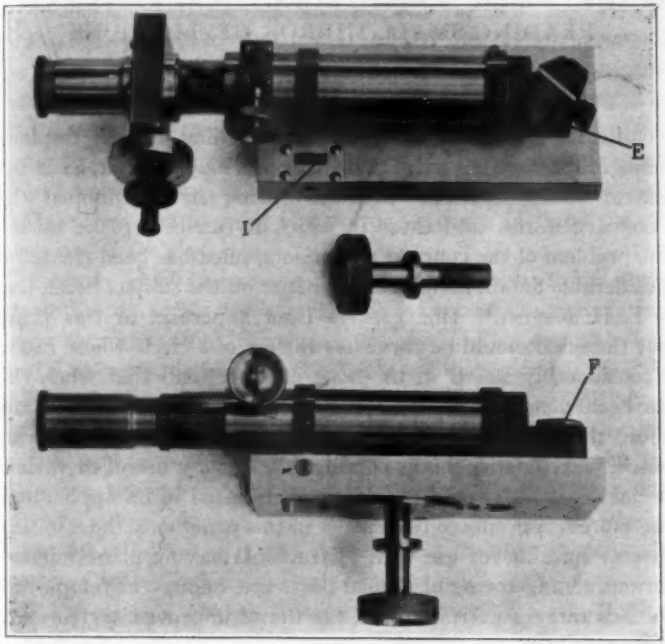

FIG. 4
The pair of instruments

and F. The slow motion screw by which the telescope is brought into parallelism is shown at I.

BUREAU OF STANDARDS,
MARCH 25, 1922

ON THE CHARACTERISTICS OF OPTICAL SYSTEMS FOR READING SMALL MIRROR DEFLECTIONS

BY
G. W. MOFFITT

Optical systems intended for use in reading angular displacements of the rotating parts of instruments may be grouped in two general classes—those in which the moving part is equipped with a concave mirror and those in which it carries a plane mirror. The problem of the concave mirror so applied has been treated in considerable detail, an interesting paper on the subject being that by E. H. Rayner.[1] One gathers from a perusal of this paper that the scale should be curved to the arc of a circle whose radius is considerably less than the scale distance, and that when this has been done the readable deflections may have large values before the optical performance breaks down. A system having these characteristics might conceivably be very useful in various special cases but would necessarily be limited in its application. Therefore it is proposed to consider in this paper only those optical systems suitable for use with instruments having plane mirrors. Various arrangements of optical parts are, or may be, employed. The advantages accruing from the use of improved systems will be pointed out.

The telescope and scale usually supplied with the ordinary galvanometer is quite satisfactory for small and moderate deflections. But while the length of scale appearing in the field of view is sufficient for the reading of steady deflections it may not be all that could be desired for the reading of ballistic excursions of comparatively short period. Moreover, if the scale be a straight one it goes more and more out of focus with increasing deflection. The blurring due to this cause may not be noticeable, for the accommodative power of the eye comes into play unconsciously,

[1] Proc. Opt. Convention, 1912.

but the parallax relative to the cross-wire remains and introduces uncertainty in the readings. Fortunately, at least in the case of many galvanometers, it is desirable to use a curved scale in order to render the calibration more nearly linear. This reduces the out-of-focus effects for large deflections but will not entirely eliminate them unless the scale be circular and with its center at the mirror. But all instruments are not galvanometers and sometimes the straight scale is the only correct one. Certainly it is the least troublesome to establish and maintain in adjustment. Thus the ordinary telescope and scale may not be as satisfactory as could be desired even in many of those cases where the dimensions of the mirror do not enter as a factor seriously affecting the performance of the system.

In nearly every instrument depending upon a central restoring couple for its action the moment of inertia of the moving part enters detrimentally. Sometimes the mirror is responsible for a goodly part of this inertia. Therefore a small mirror is desirable and sometimes absolutely necessary. To secure satisfactory results with small mirrors requires proper design of the reading system, and even when the system is properly designed the limitation on illumination imposed by the small mirror cannot be removed. For this reason a definite limit exists beyond which it is not practical to go in the reduction of the mirror size unless excessive scale illumination can be employed. It is easy to see that this is so. For in any system containing a small mirror (1 or 2 mm) this mirror will certainly be the limiting stop. From each point of the scale a certain quantity of light energy reaches the mirror. Nothing can be done after the light beam has been reflected from the mirror to increase this flux and therefore, with a given magnification, all arrangements which put this flux into the pupil of the eye will have the same theoretical image brightness. Assuming a total magnification of about two diameters, the best that can be had with a mirror 2 mm. in diameter is an image brightness about 1/40th of the scale brightness.

There remain for consideration (1) the elimination of mirror and of objective limitations on the field of view, (2) the elimination of out-of-focus effects, and (3) the control of calibration.

LAMP AND SCALE

While this paper is intended to be primarily a discussion of telescopic systems of scale reading a few words may be in order on the lamp and scale. This device has gained greatly in favor of recent years for it possesses all the convenience of projection methods in general. Unfortunately it is also afflicted with the limitations of these methods. It has comparatively low precision and in an attempt to overcome this limitation inconvenient scale distances are not infrequently employed. The problem of sufficient illumination is also troublesome if the mirror be very small, but in this respect all reading systems are closely limited. In spite of its faults it seems to serve quite well enough for ordinary purposes especially with zero methods. Small movements of a comparatively poor image may be easily detected when a precise reading of the setting would be impossible.

Personal preference and physical limitations sometimes enter into the choice of a deflection reading system. For instance, a near-sighted person would not be likely to choose the lamp and scale in preference to a good telescope and scale for with the latter he would be able to place the image where he could see it best.

The lenses used in conjunction with lamp and scale are usually simple uncorrected ones. Some improvement in definition, and therefore in precision, as well as a certain amount of control over the calibration may result from the use of properly designed corrected lenses. Even with the simple lens the correct form and position will help considerably.

SMALL ORDINARY TELESCOPE AND SCALE

The use of a small ordinary telescope may or may not result in satisfactory performance. When the scale and the objective are at approximately the same distance from the mirror the total length of scale which it is possible to see in the field of view is equal to the sum of the diameters of the mirror and of the objective. Obviously then a decrease in the diameter of these parts will produce a corresponding decrease in the available field. The width of the vignetted annular portion of the field will cover a length of the scale equal to the diameter of the objective, and it may

easily happen that no part of the field can be seen at full illumination. These effects become apparent when the dimensions of the mirror and of the objective are such that the field of view fixed by them does not fill the field of the ocular. They become serious when the mirror is smaller than that carried by the ordinary galvanometer.

MODIFIED SMALL TELESCOPE AND SCALE

Since the total available field obtainable with an ordinary small telescope may show a length of scale equal to the sum of the diameters of the mirror and of the objective one might be led to think that any decrease in mirror size could be compensated by a corresponding increase in the objective diameter. But such is not the case, for the objectives of ordinary telescopes should be, and usually are, of a diameter equal to the magnification multiplied by the diameter of the pupil of the eye. Thus the image of the objective fills the pupil and any light passing through the outer zones of a larger objective could not enter the eye.

It is, of course, well known that the image of the effective stop must fall in the pupil of the eye if stop limitations of field are to be removed. When the ordinary small telescope is used the mirror will be imaged so far forward that the pupil of the eye cannot be properly placed to receive it. The obvious remedy is to weaken the field lens of the ocular to such an extent that the image of the mirror will be brought back to the proper position. Full correction will often require the use of a negative field lens. Dispensing with the field lens helps considerably, and many of the telescopes supplied with the better grade galvanometers are so constructed. This leaves the ocular a simple magnifier with all the shortcomings of an eye-piece of that type. It is usually sufficiently good for the field of view required of it. If it does not prove satisfactory a properly designed achromat may be used instead.

Thus may the field limitations of the mirror be removed. But those of the objectives still remain. If the length of the scale visible in the field of view is not enough the size of the telescope may be increased. That is, the focal length of the objective may be increased and the telescope advanced towards the mirror, the ocular being simultaneously modified to hold the desired magni-

fication and exit pupil position. In this way the angle subtended at the mirror by the objective may be increased and the field limitations of the objective reduced or removed. It may not be necessary to increase the size of the objective to accomplish this result. The increase in general dimensions of the telescope will have a beneficial effect on the performance of the simple ocular.

BUILT-IN OBJECTIVE WITH OCULAR AND SCALE

There are limits beyond which it is not practical to go in the lengthening of a reading telescope. Moreover the tendency to go out of focus for large deflections with straight scale cannot be overcome in any of the systems already considered. Practically nothing can be done with them to modify the calibration curve of the instrument. It therefore occurs to one to do away with the traditional telescope and devise an optical system which may not look like a telescope but which will meet the desired requirements.

By the use of a correctly designed objective mounted on the instrument near the mirror in combination with a scale and a special ocular carried on the scale support it is possible to eliminate mirror and objective limitations on the field of view and at the same time to maintain the focus and freedom from parallax for large deflections, and also to gain some control over the form of the calibration curve. The problem of the design of this special objective is somewhat similar to that of the single achromat to be used with front stop as a photographic objective. The correction for the out-of-focus effects corresponds to the flattening of the field. The control of the calibration curve corresponds to the control of distortion. Just how much can be done in modifying the calibration curve actual design alone will determine.

Since the built-in objective works at practically unit magnification the demands on the ocular are light. And for the same reason the cross-wires may be made much coarser than in an ordinary telescope. If the scale is to appear twice as large as it does when viewed at reading distance by the unaided eye the focal length of the ocular will be about 10 cm. Double this power may be used if desired and still retain a good field of view.

Two ocular systems claim attention. The one consists of a simple lens of about 10 cm focal length mounted at the proper

place within a tube carrying the cross-wire at one end and the eye-ring at the other. With this very simple ocular good results may be obtained provided the field of view required is not too large. The second ocular is of the Kellner type with simple lenses of about 10 cm focal length and separation such that the cross-wires may be dispensed with and cross-lines ruled on the flat surface of the field lens instead. The advantage accruing from the use of the large Kellner ocular is the improved definition over a wide field of view which is very desirable in ballistic work. However, the field lens must be of considerable size for the image of the scale coincident with its flat face is of natural size and if 5 cm of the scale are to be seen the lens must have a horizontal dimension of the same magnitude. It need not be round however but may be narrow in the vertical dimension in order to avoid obstructing the view of the central portion of the scale. In this case the ocular housing would assume the form of a flat triangular box with the field lens as the front side and the eye-ring at the rear corner.

A scale reading device of this kind is also suitable for use with lamp and scale without change. In fact it should perform better than the ordinary lamp and scale because of the properly designed and corrected lens. It would have the same type of calibration and the flatness of field that it has when used with the ocular.

SUMMARY

1. The shortcomings of the various deflection reading devices in use with plane mirrors are considered. The use of an ordinary small reading telescope leads to a restricted field of view due either to mirror limitations, or objective limitations, or to both of these.

2. The mirror limitations on image brightness cannot be eliminated when very small mirrors are required.

3. All reasonable field limitations may be removed by proper design.

4. A special construction using a "built-in" objective on the instrument may have no limitations on field. Out-of-focus effects with straight scale are removed and a certain amount of control over the calibration realized. The system may be used with lamp and scale if desired.

ROCHESTER, N. Y.,
FEBRUARY, 1922

CONTACT RESISTANCE

BY
IRVING B. SMITH

The instrument maker's art while bound by certain conventions, has of necessity to conform to the requirements of science. Consequently there are various fundamentals of design and construction to be understood and embodied in every well conceived scientific instrument.

We are gradually ignoring many of those conventions which have in them no essential reason for existence. We have outdistanced the tailor, who still puts buttons on the backs of our coats, but we cannot, however, afford to consider only ultilitarian purposes in design and construction. There are certain constructions that remain as the hall-mark of good work; constructions that give pleasure to the conscientious maker and satisfaction to the user; constructions that bring the maker and user together with a sympathetic feeling that some things are worth while for their own sake. We still make binding posts with carefully fitted parts although the expense of doing so is not returned in better operation or more useful application. We put our best efforts into producing materials of high finish and designs of pleasing appearance as a matter of pride while knowing that necessities of service do not require such work. These points offer a means of personal expression tending to elevate the instrument maker's art to an altruistic plane. We hope they will endure.

But, as stated before, there are certain fundamentals, dictated by science and good engineering, that we must always observe if we would produce apparatus of true worth. Knowledge of these requirements should be the common property of all instrument makers and it should be the duty of each of us to aid in the dissemination of this information. These prefatory remarks are a sufficient apology for offering, in the following pages, a rather fragmentary report on the subject of this paper namely, Contact

Resistance. Contacts between similar or dissimilar electrically conducting materials enter so largely into the construction of instruments as to warrant their careful consideration. The writer has not had sufficient experience to present the subject with the completeness that it warrants but he believes that there is contained herein a considerable amount of data that will be of value to those who design or construct electrical measuring instruments.

In many cases the method of measurement employed is such that the actual resistance values of one or more contacts enter as an error in the measured result. Consider, for example, the measurement of resistance by comparison with known resistances employing the method of Wheatstone's Bridge. This process of

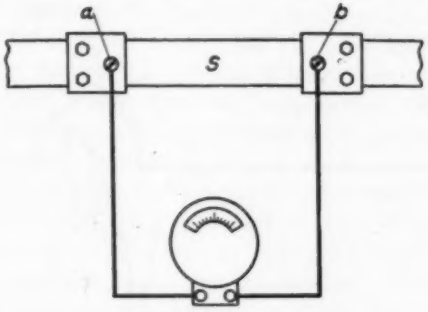

FIG. 1

Millivoltmeter and shunt for measuring heavy currents. Leads are clamped to shunt potential terminals and their resistance and also the contact resistances where attached to the shunt enter into the calibration of the meter.

comparison demands the adjustment of the standard resistances to equality with the unknown or to some determinate ratio. This adjustment may be made with plugs or switches and the result is affected by the actual contact resistances or by changes in contact resistance.

In the measurement of current, by noting the potential drop across a fixed low resistance carrying the current to be measured, errors may arise from contact resistance at the potential points or may be occasioned by a change in current distribution arising from varying resistance at the shunt terminals. For example, a millivoltmeter connected as indicated in Fig. 1 to a shunt S by

means of leads attached to terminal screws a and b will have its calibration affected if the contact resistances at a and b vary. Taking the millivoltmeter resistance including leads as 2 ohms, an increase of .001Ω at each contact point a and b would produce an error in calibration of 1/10 of 1%.

On the other hand there are methods of measurement wherein the actual contact resistance does not cause a direct error. Take, as an instance, the measurement of a potential difference with a potentiometer. Here, the contact elements consisting of switches, plugs or sliders may have a considerable contact resistance without producing an appreciable error in the measurement since the variable contacts may be so arranged as to enter into the galvano-

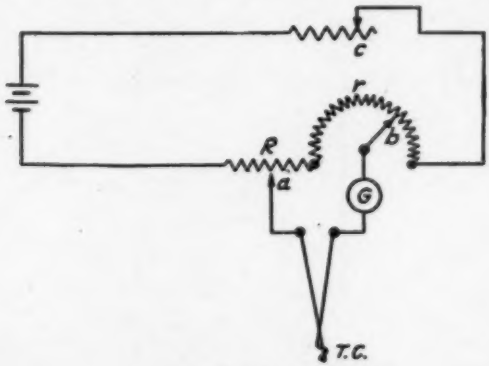

FIG. 2

Simple potentiometer circuit. Standard cell has been omitted. Contacts at a and b are in the galvanometer circuit and only introduce an error to the extent that they lessen the deflection of the galvanometer for a small unbalance.

meter circuit and not into the potentiometer circuit proper. The effect of this arrangement may be to reduce somewhat the sensitivity of the indicator but not the accuracy of the measurement. Referring to Fig. 2, which is a diagram of a simple potentiometer circuit such as one frequently employs for measuring temperature, the thermocouple T. C. is in series with the galvanometer G and the two sliding contacts a and b are adjusted until the galvanometer indicates no current through its circuit. Since the reading is taken at the time of no current through the contacts a and b, it is

not material whether these contacts vary in resistance excepting that if they increase too much in resistance they will reduce the sensitivity of the instrument and hence the precision of setting. For example, with a thermocouple circuit resistance of 40 ohms, an increase in resistance of 5Ω at each point *a* and *b* will reduce the sensitivity 25%. If, therefore, the instrument normally was sufficiently sensitive so that a variation of .01°C. was noticeable, it will now be sensitive only to .0125°C. It is evident from this that the contact variation impairs the measurement relatively but a small amount. In other cases the effect of contact resistance may be solely a mechanical one and its elimination or lessening may be demanded because of the chance of causing mechanical damage. Bus bars with insufficient area of contact, where clamped together, or where attached to other electrical apparatus, may occasion the production of an amount of heat at the junction great enough to exceed a safe operating temperature. It is evident from this brief discussion that many elements enter into the design and construction of contacts.

The writer, in line with his work for the company with which he is connected, has compiled a data book devoted to the subject of contact resistances. These data have served a useful purpose in the design of electrical measuring instruments. While it is recognized that the notes are fragmentary and the subject by no means exhaustively treated, it is hoped that a brief review of the more important items in this record may be of value to readers of this journal in presenting at least approximate values for the contact resistances usually encountered.

For convenience of reference, contacts have been classified under eight heads. This is arbitrary and more or less artificial since the various types of contact cannot be rigidly separated into classes, each possessing its own peculiar physical characteristics. For example, knife switches partake of the nature of clamped contacts when closed and sliding contacts when being opened and closed. As an approximate guide, resistance values have been set opposite various types of contact, in the following classification, to enable one to form a judgment of the probable value of the resistance to be expected.

CLASSIFICATION OF CONTACTS

A.—*Soldered, Brazed, Welded Contacts*	
Shunt Terminals	
Resistance Coil Terminals	
Thermocouples	
B.—*Clamped Contacts*	
Bus Bars	
Binding Posts	.0002Ω to .00004Ω
C. Clamp or Equivalent	.00008Ω to .00001Ω
Track Bond	.000025Ω up
C.—*Spring Contacts*	
Knife Switches, small	.0003Ω to .0001Ω
Dial Switches	.00004Ω to .0001Ω
Sliders on Wires	.012Ω to .003Ω
Sliders on Spirals	.021Ω to .00013Ω
D.—*Pressure Contacts*	
Keys	.01Ω to .0001Ω
Relays	.01Ω to .0001Ω
Knife Edges	.16Ω to .006Ω
E.—*Plug Contacts*	
Taper Plugs	.0007Ω to .00002Ω
Split Plugs	
F.—*Moving Contacts*	
Slip Rings	.5Ω to .0025Ω
Governors	.03Ω to .006Ω
G.—*Mercury Contacts*	
Copper Links	.000004Ω up
H.—*Earth Contacts*	
Rods and Plates	1000Ω to 10Ω

Ludwig Binder (Elekn. Masch. Sept., 1912) in testing between steel balls or between a steel ball and plates of copper, lead or carbon concludes that the nature of resistance at the point of contact is not that of an air or liquid film if the contacts are dry but a true ohmic resistance due to there being but a few points in actual contact. In many practical cases, however, the contact resistance arises from contamination of the surfaces in contact due to chemical action or the presence of foreign substances such as grease or dirt. This phase of the subject, however, we are not considering in the following pages.

In making the following measurements either a fall of potential method was employed or else a Kelvin Bridge method. When employing the first method the connections were as shown in Fig. 3 where the current was supplied by the battery B and measured

by the ammeter A. Potential points C and D were provided by drilling a clearance hole to the point indicated and soldering in insulated wires in such a manner that they were in contact only at their ends. The current was usually less than an ampere and the galvanometer G was calibrated to read microvolts. The battery current was reversed to eliminate Peltier effect. The resistance of the conductors between the potential points could in general be neglected. For example, when measuring the contact resistance between a brass block 1 inch in diameter and a brass plate there was less than 3/16 inch of length of brass between the potential points. Its resistance value, of about .0000008Ω was neglected.

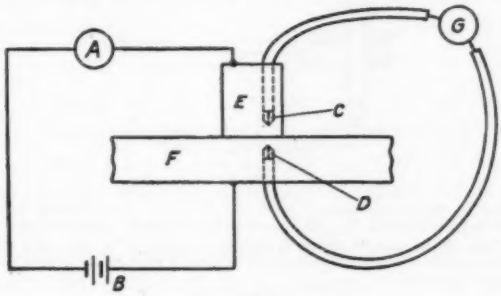

FIG. 3

The contact resistance between brass block *C* and brass plate *D* is determined by measuring the current through the contact by means of the ammeter *A* and the potential drop across the contact by galvanometer *G*. The block *C* was of round brass 2.5 cm in diameter milled off square and polished with emery cloth. The brass plate was commercial rolled plate with face polished with emery cloth.

In the determination of the contact resistance of switches and binding posts a similar method was employed. In measurements on binding posts the potential points were arranged as indicated in Fig. 4. The potential wires were of silk insulated copper No. 32 B. &. S. Gage and the clearance holes small in order to change the current flow lines as little as possible. It will be noted that in this method the contact resistance measured is really that of two resistances in series whereas in actual use the two resistances are in parallel. It is assumed that a fair measure of contact resistance will be gotten by dividing the measured value by two rather than by four.

In the determination of the contact resistance of plugs the Kelvin Bridge method was used. The connections were in accordance with Fig. 5 where holes were drilled in the brass blocks at C and D to such a depth that the bottoms were within 1 mm of the

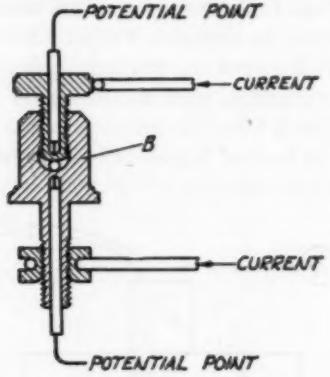

FIG. 4

In measuring the contact resistance of binding posts the current passes through the screw and wire clamped under the post, thence through the body of the post. Potential points are placed, one near the bottom of the screw and one near the bottom of the hole in the body of the post. It is assumed that the contact resistance so measured is double the value to be found in the normal use of the post.

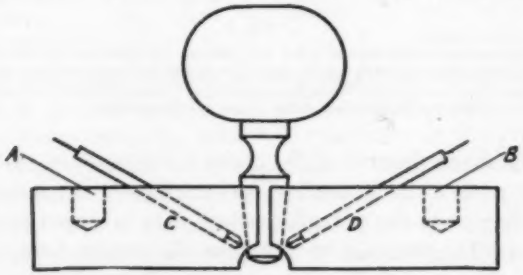

FIG. 5

In measuring plug resistances the potential points are brought close to the plug surface at *C* and *D*. The current terminals are at *A* and *B*.

surface of the plug. A very small amount of mercury was placed in each hole and insulated copper wires made contact only at their ends with the mercury. These served as potential points. The current entered the mercury cup at A and left at B. The measure-

ments were generally taken with a small current since most of the apparatus was for use in electrical measurements where the current values were practically always small. Variations in the measuring current, however, from ½ to 3 amperes produced only a small change in the contact resistance measured. For example, in measuring the contact resistance of a phosphor bronze laminated dial switch, the measuring current was varied from 1 ampere to 2.5 amperes resulting in a change in contact from .000175 to .000184 ohms.

It was concluded from this and similar measurements that for the purposes of this investigation the variation of resistance with the current could be neglected since the current in actual service was in general small.

A. SOLDERED, BRAZED, WELDED CONTACTS

Pieces of round brass and copper rod were squared on the milling machine and joined butt on with commercial soft solder. The solder itself 2.5 mm in diameter had a resistance of .0003ω per cm of length and as an element in the joint resistance of two quarter inch rods would be less than .8 microhm. Knife edge potential points at a fixed distance apart, were placed first so as to span a section of the rod not including a joint and then so as to include a joint. For two 6 mm diameter brass rods the joint resistance measured .000003Ω. For two 3 mm diameter copper rods the joint resistance measured .000004Ω. These measurements should have been repeatedly checked but time prevented. They will, however, serve to give one an idea of the order of magnitude of such resistance.

B. CLAMPED CONTACTS

To study the effect of pressure on clamped contacts, a number of brass blocks were squared off carefully and pressed against a smooth brass plate. The pressure was varied by means of weights and the area of apparent surface contact was varied by employing blocks of different diameter. The actual areas of opposed surfaces do not, of course, determine the area of the contacting parts since it is impossible so to machine the surfaces as to insure contact at all points. It is, therefore, difficult to determine the true

relations between contact resistance, area and pressure. However, most curves have the form shown in Fig. 6 where the resistance is comparatively high for light pressures and bears a nearly linear relation to the pressure until some value is reached where subsequent increase in pressure does not lower the resistance greatly. Curve A was obtained with surfaces that were polished just before testing. Curve B was for the same surfaces exposed to the atmosphere over night. The materials in contact were respectively a large brass plate and a brass block 2.5 cm in diameter. This in-

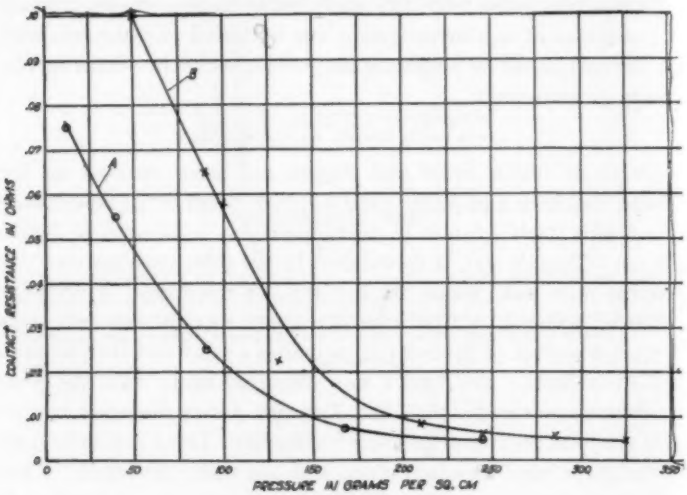

FIG. 6

These curves were the results of tests on the metal blocks shown in Fig. 3. The current was held steady at a known value and the fall of potential measured by a galvanometer. The blocks were both of brass.

dicates that at a pressure of about 200 grams per sq. cm of apparent contact surface the contact resistance reduces to .005Ω. Additional experiments with more accurately finished contact surfaces resulted in reducing the contact resistance to .00005Ω for a pressure of 800 grams per sq. cm. A considerable variation in resistance was found for different tests. These variations could in general be eliminated by moving the contacting block sidewise while the pressure was on. This adds weight to the usual recom-

mendation that before finally setting up a clamped contact one of the elements should be driven sidewise slightly.

Binding Posts.—Binding posts are a form of clamped contact used perhaps more often than any other and more frequently employed where their actual contact resistance enters as an error in the measured quantity. If the contacting surfaces are clean, binding posts, when well screwed down, show a resistance usually much lower than the copper leads used to connect them in circuit. For example, two copper leads each of No. 8 B. & S. wire and each two feet long would offer a resistance of about .0025Ω. A pair of binding posts suitable for No. 8 wire, if clean and well clamped, would present a resistance of about .00008Ω. The danger in the use of binding posts resides of course in the fact that they may under various circumstances offer a fairly high resistance. If they are small or not firmly screwed up or dirty a materially higher resistance value results.

Running over all the values given in the writer's data book for large binding posts, small ones, posts with holes for the reception of either wire or flat stock and considering copper, brass, and nickel plated posts, the general conclusion may be reached that the contact resistance varies from .0008Ω for dirty and oxidized parts clamped together tightly to .00004Ω for clean surfaces firmly clamped. The figures of course vary considerably but assuming that reasonable care is taken to clean and clamp the wire or terminal into a binding post there should be no difficulty in keeping the contact resistance always below.0002Ω. In exceptional cases where heavy clamps are used instead of binding posts the contact resistance can be kept below .00001Ω. Flat grip binding posts particularly if not kept clean offer a higher resistance than the type of post where the connecting wire enters a hole and is clamped with the end of the binding post screw. The latter type of post tends to clean the contact elements as the screw is turned down. Brass binding posts offer a less resistance when clean than nickel plated ones but if not kept clean the nickel plated ones show a lower resistance.

C. SPRING CONTACTS

Under spring contacts we shall consider knife switches, dial switches, slider contacts, commercial connectors and generally, contacts whose elements are held together by spring pressure.

There is prevalent a belief that a switch, one element of which has many leaves or laminations is sure to have a low contact resistance. But such a conclusion is not warranted unless the individual laminations are so far independent as to permit each one to operate without interference from the rest. As a rule, when a switch has a large number of leaves, mechanical considerations require that they shall be so close together as to either touch each other or offer small crevices for the lodgment of dirt, particles of lint, etc. These are gradually worked up into the brush until finally the brush operates as though it were one solid structure; and moreover, the material forced between the laminations has lifted some of them so that the contacting face is no longer in proper bearing. On the other hand a laminated brush possessing say two or three or at most a half dozen leaves offers no serious mechanical difficulty in the way of proper spacing of the leaves with the result that such a brush shows consistently low in contact resistance, assuming of course that it is properly constructed, being more likely to improve itself with age because of the wearing in and more intimate fitting of the various parts. We have had occasion to measure the brush resistance of switches possessing a large number of laminations packed so closely as to prevent individual leaves from having really independent motion and have found the resistance to be higher than that of brushes of much simpler construction, and moreover, they tend to increase their contact resistance as stated above because of their picking up and holding lint, copper dust, dirt etc. When new, a switch of this type is low in resistance but as it wears it is likely to show a higher resistance. Another point for consideration is that when the laminations are closely packed and are in considerable numbers the brush has practically no elastic recovery. As a result the smallest wear eases up on the brush contact pressure and adds to the contact resistance. It has also been noticed that, when a brush of this type is mounted on a hard rubber plate, the continued

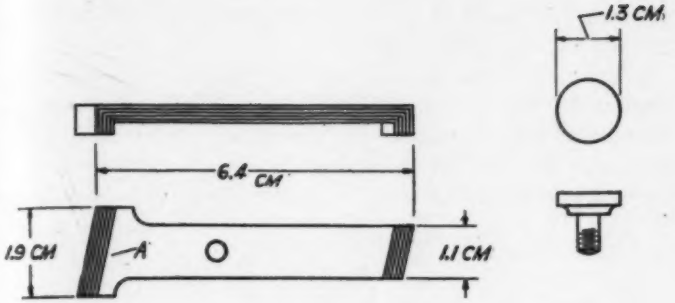

FIG. 7

Brush No. 102

Six phosphor bronze laminations .4 mm thick

For contact resistance at end *A* see Fig. 14, curve *A*. With this brush there was employed contact stud No. 87, Fig. 12. Stud was of brass. Normal pressure is such that the contact resistance is about .0002Ω at each end of brush. The same combination was tested when the stud was nickel plated. See Fig. 14, curve *B*. Normal resistance about .0004Ω at each end of brush.

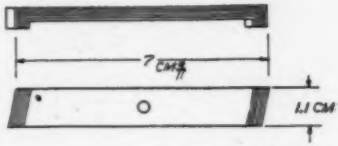

FIG. 8

Brush No. 101

Six phosphor bronze laminations .4 mm thick

For contact resistance at one end, see Fig. 14, curve *C*. With this brush there was employed contact stud No. 86. Nickel plated. See Fig. 13. Normal pressure is such that the contact resistance is about .0004Ω at each end of brush.

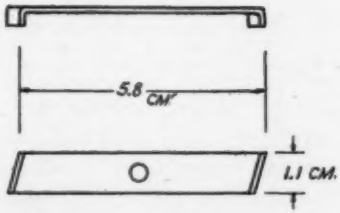

FIG. 9

Brush No. 100

Two phosphor bronze laminations .8 mm thick

For contact resistance at one end, see Fig. 14, curve *F*. With this brush there was employed contact stud No. 87 nickel plated, Fig. 12. Normal pressure is such that the contact resistance is about .0008Ω at each end of brush.

pressure has caused the rubber to give sufficiently to materially increase the contact resistance.

The contact resistances of various types of switches are indicated under Fig. 7 to 13 inclusive, together with curves showing the relation between pressure and resistance. Referring to Fig. 14 the conditions under which curves A, B, C, D, F and H were gotten are indicated under Figs. 7 to 13. Curve E was for the same brush

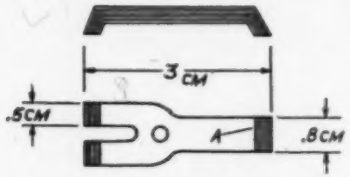

FIG. 10

Brush No. B

Nine phosphor bronze laminations .4 mm thick not spaced apart

For contact resistance at end *A* see Fig. 14, curve *H*. With this brush there was employed stud No. 86, Fig. 13. Stud of brass not nickel plated. Normal pressure of about 4 kg per sq. cm results in a contact resistance of about .0003Ω.

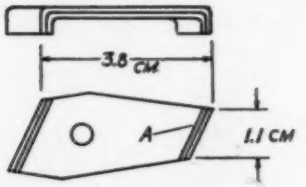

FIG. 11

Brush No. S

Three phosphor bronze laminations .4 mm thick

For contact resistance at end *A* see Fig. 14, curve *D*. With this brush was employed contact stud No. 87, Fig. 12. Stud was of brass. Normal pressure such that contact resistance at end *A* is about .0002Ω.

and stud as shown in Fig. 7 excepting that the stud was of bare copper. Curve G was for the same brush and stud also as indicated in Fig. 7 but the stud was of bare brass. It differed from the conditions of curve A only in the fact that it was new whereas the brush and stud corresponding to curve A had been used for some time.

In most instances we are interested in the variation in the contact resistance rather than in its actual value since the cali-

bration or adjustment of an instrument often takes account of the absolute value of the resistance at contacts but cannot compensate for variations in such resistances. With the switch parts used in determining curve C the contact resistance for a normal pressure

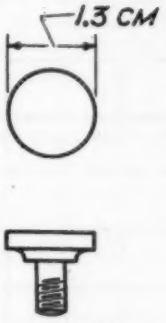

FIG. 12

Type of brass or copper switch stud employed. A potential point was located just under the top surface of stud.

FIG. 13

This stud differed from that shown in Fig. 12 in that it was of smaller diameter.

of 2 kg was about .0003Ω and its variation approximately .00006Ω. Brush pressures normally run from 1.5 to 3 kg. Contact resistances, therefore, may be expected to range from .00045Ω to .00015Ω.

KNIFE SWITCHES—These are frequently employed in small sizes on measuring instruments. A 15-ampere commercial knife switch old and not used for some time varied in resistance from .0001Ω to .0003Ω. A similar but new one varied from .00007 to .0001Ω. A third familiar form whose clips were bent back on themselves varied

from .00019Ω to .00024Ω this high resistance being due to the fact that the doubled length of clip prevented securing an adequate pressure against the knife blade.

SMALL CONNECTORS.—The usual type of connector used in automobile head lights had a contact resistance varying from .003 to .007Ω.

FAHNSTOCK CLIP.—When clamping a piece of nickel plated copper wire, No. 12 B. & S. Gage, Fahnstock clips varied in contact resistance from .00005Ω to .0005Ω depending upon the material of the clip and its size.

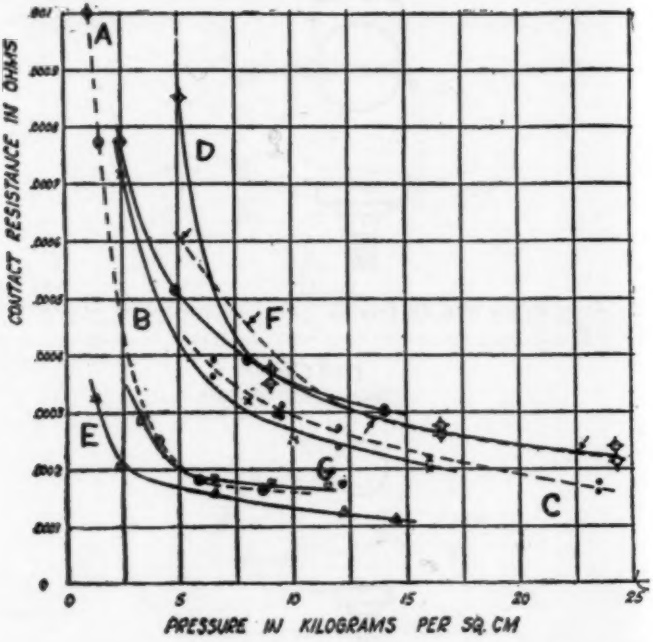

FIG. 14

The above curves were obtained with switches of various types. The previous Figs. 7 to 13 inclusive give the principal dimensions of these switches and reference is made by letter to the corresponding curve in Fig. 14.

SLIDER CONTACTS.—For many purposes a knife edge or small surface is used to make contact against a slide wire or a wire helix and it is held in contact with a nearly constant spring pressure. Motion occurs between the slider and the wire but the resistance of the contact enters for consideration only when the slider is at rest. As a rule, devices of this nature are employed in such a manner that the effects of contact resistance do not enter into the measurement excepting as they indirectly influence it by affecting the sensitivity of the indicating device. For this reason a fairly high contact resistance is often permissible.

Various materials have been used for the slider and for the slide wire or spiral. The following conclusions from a large number of tests were reached.

1. A Copper gauze slider tends to clean the wire upon which it slides.
2. A Steel slider is more variable than a gauze one but in some instances is to be preferred because of its better wearing qualities.
3. A Silver slider is best from the standpoint of low resistance and constancy but wears rapidly.

To review even briefly the numerous tests that have been made on slide wires and spirals would require too much space. A few figures, therefore, will be furnished to give one an idea of the range of contact resistance to be expected.

1. Steel Slider on Straight Therlo Wire.—Pressure 37 grams. Contact resistance varied from .006Ω to .05Ω when dirty and from .006Ω to .012Ω when cleaned by rubbing off with a piece of cloth.

2. The same combination but allowed to become very dirty by standing in a furnace room for five weeks. Without removing the dirt the contact resistance reached .3Ω. When wiped off the highest value was .03Ω and a more careful cleaning reduced the resistance to .008Ω maximum.

3. A similar test to No. 2 for a steel slider on manganin wire showed contact resistances as high as .12Ω after wiping clean indicating that Therlo is better as far as this characteristic goes than manganin.

4. Silver Slider on Therlo Wire.—When new and clean the contact resistance varied from .0017Ω to .003Ω and when dirty the variation was from .0025Ω to .008Ω.

5. Copper Gauze Slider on Therlo Wire.—When new and clean the contact resistance varied from .002Ω to .003Ω and when dirty the variation was from .003 to .5Ω. This excessive resistance was due to dirt accumulating in the meshes of the copper gauze. After filling the gauze the variation was from .002Ω to .003Ω as it was when new.

6. Silver Slider on Silver Wire.—When new the contact resistance varied from .00013Ω to .0002Ω and while dirty it varied from .00013Ω to .00022Ω. After about two months standing the resistance varied from .0003Ω to .0009Ω.

Various other combinations were tried out but those cited above give one an idea of the contact resistance to be expected. In the above cases the slider pressure against the slide wire was about 40 grams. It is an interesting fact that if the slide wire is in the form of a closely wound helix the cleaning action of the slider as it moves over the wire from turn to turn is better than if the slide wire were straight.

D. PRESSURE CONTACTS

Under this rather vague title we include contacts which are held together by finger pressure or by electromagnetic devices, such for example as keys and relays. We will refer only briefly

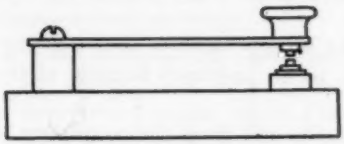

FIG. 15

This conventional type of key had silver contacts about 2 mm in diameter. The potential points were determined by drilling very fine holes in the silver contacts near their interface and peening copper wires .1 mm diameter in the holes.

to these. In an old standard type of key, Fig. 15, with silver contacts the following contact resistances were observed:

Before cleaning:

Pressure kg	Resistance Ω
.1	.085
.45	.0038
3.20	.0006

After cleaning (with fine emery):

Pressure kg	Resistance Ω
.1	.00095
.45	.00037
1.80	.00023
3.20	.0001

A telephone jack showed a contact resistance of .0025Ω for one spring and .0075Ω for another. These contacts were relatively high because the contacts are so constructed that the contact surfaces come together without sliding and therefore without any cleaning action.

Another interesting test was on a galvanometer used as a relay to close an electric circuit when the galvanometer pointer deflected against a fixed contact. Both contact elements were of platinum. The results are best shown by the curve in Fig. 16. It

might be well to add that the values shown in the curve were for clean contacts and they would be materially affected by even a small amount of dirt.

E. PLUG CONTACTS

The resistance of a plug contact varies, of course, with its condition, its construction and the degree of pressure exerted in seating the plug. Fig. 17 gives one an idea of the contact resistance of a

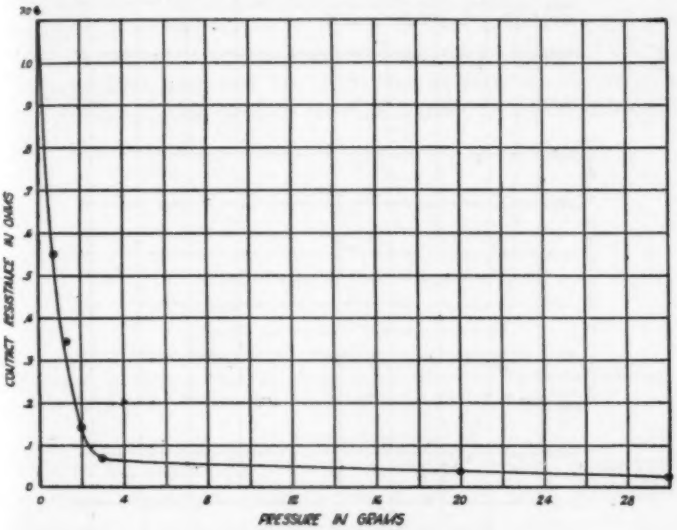

FIG. 16

Contact resistance between two crossed platinum wires of No. 22 B. & S. Gage. One wire was a pointer on a galvanometer used as a relay and the other wire acted as a stop to close a circuit when pointer was sufficiently deflected.

large taper plug in good condition set in with various degrees of pressure. For this test a plug and blocks were used of the dimensions indicated in Fig. 18. The potential points and current points were as shown in Fig. 5. While the pressure was applied and before each reading, the plug was given a slight twist. There is an appreciable reduction in contact resistance up to a pressure of 8500 grams after which the gain is small. At that pressure the resistance is about .000025. This is really a measure of two contacts

in series since the current path was from block A to block B in Fig. 5. Somewhat lower values than those shown on the curve of Fig. 17 were gotten in other tests but a fair allowance for the

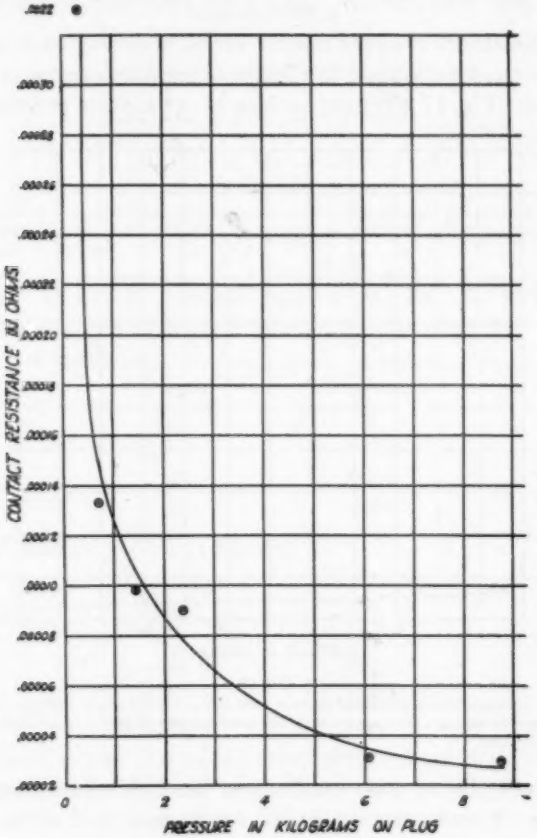

FIG. 17

Contact resistance between a standard taper plug and its block. In making the measurements plotted here the potential points were so located that two contacts were in series. See Fig. 5.

contact resistance of a large plug well fitting and firmly seated with a twisting motion is about .000025. To give one an idea of the resistance variation under these conditions the plug was taken

out and reseated before each instrument. Of thirteen measurements the mean value was .0000212Ω with variations ±.0000007Ω.

By way of comparison a similar plug was measured both when dirty and after reaming the hole and cleaning the plug. When dirty the resistance varied from .0004Ω to .0008Ω. After cleaning it varied from .0000205Ω to .0000251Ω. This variation of ±.000005 is more likely to be encountered than that given above. From this and other tests it appears reasonable to assume the resistance of a good plug contact to be about .000025Ω±.000005Ω. A plug of the above size is normally set with a pressure around 12-13 kg. For a smaller plug and for blocks not capable of withstanding such pressure, the contact resistance may be about .00005Ω± .00001Ω.

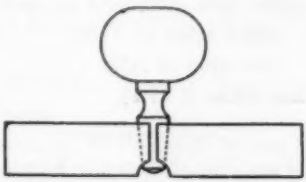

FIG. 18

Standard taper plug used in making measurements plotted in Fig. 17. The drawing represents actual size of plug and blocks. The plug taper was 1 mm increase in the diameter per cm length.

F. MOVING CONTACTS

From the standpoint of the instrument maker continuously moving contacts are of interest when carrying small currents and his interest rests more upon the constancy of resistance than the current carrying capacity. An illustration related to the instrument maker's art is that of slip rings and brushes used on rotating sockets in photometric measurements. The following is a test on such a piece of apparatus.

Contact resistance of slip ring

Slip rings brass.
Brushes copper gauze.
Pressure 150 grams.
Standing .0019Ω.
180 rpm. .026Ω.
450 rpm. .035 to .05Ω.

A phosphor bronze leaf brush of 6 leaves was substituted.
Pressure 200 grams.
Standing .0022Ω.
180 rpm. .0025Ω.
350 rpm. .0029Ω.

The contact resistance of phosphor bronze brushes on a copper slip ring was found to be $.0045 \pm .001\Omega$ when rotating at 80 rpm. and .0004Ω when standing. In this case the brush consisted of 3 leaves of .4 mm phosphor bronze 12 mm wide having a total brush pressure of 2 kg.

Contact resistance of bearing

A half inch cold drawn steel shaft running in a brass bearing 1¾ inches long at 2000 rpm. showed a contact resistance

when oiled of .140Ω
not oiled of .05Ω.

The bearing load was about 2.25 kg.

G. MERCURY CONTACTS

In Reprint (Sci. Pap.) No. 241 of the Bureau of Standards the subject of mercury contacts is considered. The resistance at the contact of one mercury contact may by proper design and construction be reduced to about .000004Ω. When in poor conditions, however, owing to an accumulation of dirt a much higher value may result.

H. EARTH CONTACTS

The conditions may vary to such a degree that it is difficult to fix any limits on the resistance of earth contacts. Values were gotten as low as 10Ω when using ground plates 12 inches square in well watered earth and as high as 400Ω when using a half inch bright copper rod.

THE LEEDS AND NORTHRUP INSTRUMENT CO.,
PHILADELPHIA, PA.

AN APPARATUS FOR DEMONSTRATING THE ELECTRICAL PROPERTIES OF CONDUCTING GASES

BY
JOHN ZELENY

The apparatus to be described permits the easy demonstration in a few minutes of the following properties of conducting gases.

1. Persistence of conductivity after removal from agent producing it.
2. Gradual loss of conductivity with time. (Recombination of ions.)
3. Destruction of conductivity by passage of gas through an electric field.
4. Destruction of conductivity by passage of gas through a plug of glass wool.
5. Loss of conductivity by diffusion of ions to adjacent walls.
6. Difference in diffusion of positive and negative ions.

APPARATUS

The apparatus is shown in Fig. 1. An open cylindrical tube A, 10 cm long, carries a central electrode B which is insulated from the tube by an amber plug and is connected to the leaf of an oscillating leaf electroscope,[1] which is most conveniently used as an indicating instrument. The tube C telescopes into tube A and may be pulled out more or less as required. Air or some other gas may be blown through the apparatus by attaching rubber tubing to the inlet tube M. An offset D in C contains a plate E on which some radioactive material is placed, which serves to ionize the gas in the tube above. A convenient radioactive material for this purpose is a discarded radium emanation tube as commonly used in radium therapy. To make use of alpha rays the tube is broken into fragments which are sealed to the plate E with the inside surfaces pointing outward. F is a short open auxiliary cylinder which may be moved by means of the rod G and its handle H, so

[1] J. Zeleny. Physical Review, *32*, p. 581, 1911.

as to occupy a position either to the right or to the left of D. F is used to hold either the cylinder I, which contains a plug of glass wool or the cylinder J which is compactly filled with wires 0.5 mm in diameter. Each of these cylinders carries a wire handle K, and a pin L which fits into a right angle groove in the left end of F (shown by dotted line in upper figure) and is thereby kept in position when air is blown through C. The short tube N, which

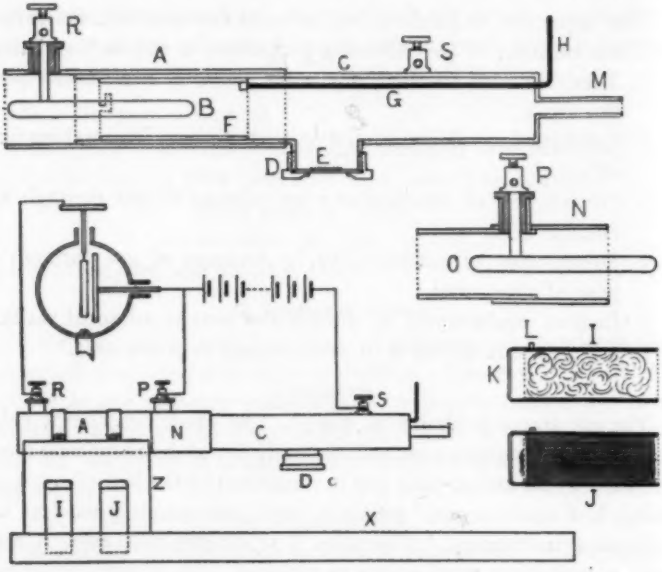

FIG. 1

may be inserted between A and C, contains an insulated electrode O with the binding post P. The apparatus is conveniently mounted on a wooden block Z attached to a wooden base X as shown in the lower left hand drawing.

PROCEDURE

1. Connect R to the plate and S to the case of the electroscope. The latter shows no action until air blown through M carries the ionized air from the neighborhood of D to B.

2. Keeping the stream of air constant, note qualitatively the rate of motion of the leaf of the electroscope. Next draw C out of A as far as possible, without breaking the union, so that a longer time is required for the gas to pass from D to B. The conductivity is much diminished owing to recombination of ions, as is made evident by the smaller frequency of oscillation of the leaf.

3. Insert N between A and C and blow gas through the tube. When P is connected to S, the gas reaching B is conducting; whereas if P is connected to the end of the battery charging the plate of the electroscope, (these connections are shown in the lower left hand drawing) thus producing an electric field between N and O, the gas coming to B is non-conducting. This arrangement can also be used to measure the mobilities of the two ions by measuring the mean velocity of the gas stream and applying different voltages to P until one is found which just permits some of the ions to get by O.

4. Remove N and place I into F. Starting with F to the right of D, the gas reaching B is conducting, whereas when F is pushed by means of H to a position at the left of D, the gas has to pass through the glass wool and loses its conductivity completely.

5. Remove I from F and replace it by J. With F to the right of D the gas reaching B has a conductivity indicated by the motion of the leaf. When F is pushed to the left of D the gas has to pass through the narrow channels between the wires which fill J and many of the ions diffuse to the walls and discharge themselves, and a marked decrease in conductivity is noted, although the time taken for the ions to reach B is somewhat diminished and there is consequently less loss of ions by recombination.

6. In the last case if the time required for the electroscope leaf to make any arbitrary number of oscillations is measured first when positive ions are pulled towards D and next when negative ions move towards B, it is found that the number of positive ions (in air) which get through the diffusion cylinder J is greater than that of negative ions owing to the greater mobility of the latter.

SLOANE LABORATORY,
YALE UNIVERSITY,
APRIL 13, 1922

REVIEWS AND NOTICES

Report on Series in Line Spectra. By Professor A. Fowler, The Physical Society of London, 13/—. Bound in Cloth 16/—. 182 pages, 12 figures, 5 plates.

"Although the spectra of elements and compounds were studied in the first instance chiefly as providing a powerful means of chemical analysis, it has long been recognized that a spectrum must contain an important clue to the structure and modes of vibration of the atoms or molecules which produce it. . . . The analysis of spectra has thus become one of the main objects of modern spectroscopy, stimulating the experimentalist to the extension of existing data, and providing material in a form suitable for the theoretical investigator."

"My purpose in the present report has been to give a comprehensive account of the development and present state of our knowledge of the regularities in spectra, as deduced from the spectra themselves, with but little regard to theories of their origin. The report is in two parts, the first of which gives a general account of spectral series, excluding those which occur in band spectra, while the second is intended to include the most authentic experimental data available in April, 1921."

The above sentences from the author's Preface briefly give the reason, purpose and scope of his Report on Series in Line Spectra which may no doubt be considered as the most timely and valuable contribution to spectroscopy in many years. The general account of series (Chapters I to IX) is a splendid introduction to the tables. In successive chapters, the Observational Data are outlined, an Historical Note 1869-1879 reviews the early discoveries, the Characteristics of Series are described in some detail and various Series Formulae, Rydberg's and others, are presented. Three chapters of more theoretical interest deal with Spectra and Atomic Constants, The Work of Hicks and Applications of Bohr's Theory. Two Appendices to Part I contain practical suggestions for the calculation of formula constants and Tables for computations.

Part II (Chapters X-XXI) devotes its first chapter to a summary Explanation of Tables, the spectra of hydrogen and helium because of unusual features are discussed in Chapter XI, and the following chapters deal with the series and general spectral characteristics of successive groups or sub-groups of similar elements according to their classification in the Periodic System.

Our knowledge of series was collected by Dunz (Dissertation, Tubingen, 1911) and Lorenser (Dissertation, Tubingen, 1913) using the notation proposed by Prof. Paschen, and for the past decade these tables have been of great service to spectroscopists. They have long required revision since something has been added to our knowledge of series, especially among enhanced lines, since 1914, and the observational data have been improved for nearly all spectra. Prof. Fowler's report not only fulfills the need of spectroscopists for accurate and comprehensive tables but also carefully analyzes many of the questions and problems involved and suggests lines of research which should give an impetus to the development of this subject.

It seems unfortunate, however, that no uniform and strictly consistent notation has been thus far agreed upon for the naming and representation of series. That used by Paschen and his followers has been widely quoted in connection with the recent developments in quantum theory of spectroscopy and the Ritz formula has been derived from theoretical considerations. In Fowler's tables, the Hicks formula is preferred and the constants, including the computed limiting frequencies of series, are different from those given by the Ritz formula and the numeration of the lines is different.

Four chief series are generally recognized, the three first discovered being named the Principal, Diffuse and Sharp series while the fourth, first observed by Bergmann in 1907, is often referred to as the Bergmann Series, but some writers follow Hicks in calling it the Fundamental Series. The latter word appears unfortunate since it conveys an idea of importance like that suggested by the word Principal and the series is probably less fundamental than any of the others. Physicists who have adopted the Paschen system will find little difficulty in translating one notation into the other if the important differences are compared. The four chief series are written in abbreviated notation as follows:

Fowler	Paschen
P(m) = 1 S − m P, m = 1, 2, 3	P(m) = 1 S − m P, m = 2, 3, 4
S(m) = 1 P − m S, m = 2, 3, 4	S(m) = 2 P − m S, m = 3, 4, 5
D(m) = 1 P − m D, m = 2, 3, 4	D(m) = 2 P − m D, m = 2, 3, 4
F(m) = 2 D − m F, m = 3, 4, 5	B(m) = 3 D − m B, m = 4, 5, 6

It should be noted that there is some inconsistency in the convergence frequency of F(m), which is sometimes 2 D and in the other cases 1 D while for B(m) it is 3 D.

In order to distinguish series of different kinds, Paschen designated singlet systems by capital letters, doublets and triplets by small letters and series among spark lines by German capital letters. Prof. Fowler adopts Greek letters for doublets and small letters for triplets. Thus:

Fowler	Paschen
P, S, D, F = Arc Singlet system	= P, S, D, B.
π, σ, δ, φ = Arc Doublet system	= p, s, d, b.
p, s, d, f = Arc Triplet system	= p, s, d, b.
π, σ, δ, φ = Spark Doublet system	= $\mathfrak{P}$, $\mathfrak{S}$, $\mathfrak{D}$, $\mathfrak{B}$.

Satellites are designated by primed letters in both systems. Perhaps neither of the above notations is entirely satisfactory. In the interest of economy in publications both the German capital letters and the Greek letters should be eliminated and large and small Latin letters retained as sufficient. It is perhaps advisable to distinguish more carefully hereafter between arc and spark spectra, i.e., those arising from neutral atoms and those from atoms which have lost one or more electrons. Fowler indicates that in the cases of carbon and silicon in addition to the arc spectrum, the first, second and third spark spectra have probably been detected. Such enhanced spectra will no doubt be of still greater importance in the future and it seems appropriate to characterize the series as s^+, etc., in a manner analogous to that in which the ionized atoms are symbolized.

Prof. Fowler hopes "that the tables of series lines, together with the references to lines which have not yet been classified, will suggest and facilitate further investigations." The tables show that of the 87 chemical elements whose spectra are more or

less well known, only 25 have thus far disclosed series, properly so-called, in their arc spectra and of these only 5 (or possibly 11 if the meager data on Ra, Zn, Cd, Hg, C, Si be considered) have had some of their spark lines organized into series. Aside from the advance in this class of series which have come in recent years principally from Fowler, there has been very little progress in the arrangement of spectra into series since the work of Kayser and Runge and of Rydberg over 30 years ago.

It is perhaps not an overstatement to say that in this period more irregularities have been discovered among so-called series than regularities in other spectra. The subject does not appear as simple now as it once did. Fowler has called attention to most of these special features in a discussion of negative wave-numbers, a chapter on "Abnormal" series (Chap. VI) peculiarities of the diffuse series of Al and of Mg, the P series of Ca, etc. The Rydberg number N which was once thought to be a universal constant now appears to be different for each chemical element. In addition to the regular series which in general account for only a part of the total number of spectral lines observed for an element, other types of regularities such as the "combination series" first discovered by Ritz, inter-combinations, and unclassified pairs or triplets with constant differences in frequencies are also found. In many spectra where no sequences have yet been detected, there are pairs or groups of lines with constant wave-number separations, which may possibly have their origin in combination terms between parallel series as has recently been shown by Paschen to be the case with Neon. With the single exception of Neon, practically all of whose 900 lines have been skillfully arranged in about 132 series, our knowledge of series is still confined to the simpler spectra, and even in some of these many of the observed lines are still unclassified. Elements of the iron group, the platinum metals, rare earths and others whose spectra are a wilderness of many thousands of lines must wait patiently for the discovery of series. In such complex spectra it is very difficult to detect related groups or to find a starting point for the series. The displacement law of Kossel and Sommerfeld is suggestive as to the kind of series to be expected in the spectrum of each element according to its position in the periodic table and a possible numerical relation between the spark spectrum of an element and the arc spectrum of the element which precedes it in the periodic table has been suggested by the same writers. Many investigations have been made to connect the progressive change in spectra with atomic weights, atomic volumes or atomic numbers, but the exact laws governing the changes are still unknown. It appears that for the present the "cut and try" methods of arranging observational data offer the chief hope for the disentanglement of complex spectra, with the possible assistance of further experimental results on the Zeeman effect, radiating potentials, the sensitive lines of spectro-chemical analysis, etc. For the development of such work, Prof. Fowler's report will be of enormous benefit. It points out the best data, gives a complete statement of our present knowledge, suggests problems which need further investigation and opens the field to all. The only severe criticism which can be offered is on the lack of an index to the vast material of this report. The table of contents is inadequate as a reference to the wave-length tables and remarks relating to the individual chemical elements.

W. F. Meggers